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THE MANUFACTURE OF TITANIUM[1]

by

Dr. JAMES TAYLOR, M.B.E.

Introduction

In recent years a number of metallic elements have ceased to be rare laboratory specimens and have been produced in industrial quantities. Uranium and the other fissile materials are outstanding examples, but there are others. For instance, the remarkable electrical properties of germanium have led to the development of the transistor, which bids fair to supplant the familiar filament valve. And now the combination of high melting temperature, excellent corrosion resistance and low neutron absorption of the metal zirconium is leading to its employment in the atomic energy field, and production facilities are expanding rapidly. Beryllium and niobium are other examples.

The industrial birth of these metals has been associated with problems which have required the development of radically new techniques, or the scaling up of procedures hitherto only employed in the laboratory. Thus germanium for transisters must contain only one part in a thousand million of impurity, a fairly tight specification which was only achieved by the development of the new zone refining technique. Zirconium for atomic energy must be free from hafnium, and one solution of this classical separation problem has invoked the large-scale operation of liquid–liquid extraction.

However, the metal which I wish to take today as exemplifying the application of some new techniques in metallurgy, is titanium. This metal possesses the strength of steel, but is only a little over half as heavy. This combination of properties has led to its extensive use, particularly in military aircraft in the United States, in jet engines, fireproof bulkheads, firewalls, ducting and shrouding, and, in this country, now that supplies are available, similar applications are forging ahead. It is foreseen that the metal will be of great value in the supersonic-jet era which we are just entering.

Titanium shows magnificent resistance to rusting and corrosion, and its outstanding resistance to seawater, for example, makes it certain that applications utilising this property will be developed before long.

Titanium is being built into modern aircraft in rapidly increasing quantities, and already its scale of manufacture far exceeds that of the other metals I have mentioned.

Its production is dominated by its great affinity for oxygen, nitrogen, hydrogen and carbon, and by the disastrous effects which these impurities produce on its ductility and toughness. Thus carbonaceous smelting of titanium oxide ore is quite impracticable and resort has to be made to reduction of titanium tetrachloride by magnesium or sodium.

Titanium Tetrachloride Production

I must touch rapidly on the manufacture of titanium tetrachloride, which is the first step in the production of the metal.

Titanium occurs in nature as the dioxide TiO_2, which is present in small quantities almost everywhere. The useful ores, however, are rutile, which is a fairly pure form of the dioxide, and mixtures or solid solutions of titanium dioxide with ferric oxide, notably ilmenite ($FeTiO_3$). Large deposits of massive ilmenite occur

[1] Paper delivered to Section B (Chemistry) on August 30, 1956, at the Sheffield Meeting of the British Association.

chiefly in North America, India and Norway, while rutile is generally obtained from beach sands in Australia and India.

The first step in the extraction of titanium metal consists in passing chlorine through (that is to say chlorinating) a mixture of the ore and coke to produce titanium tetrachloride, which can readily be purified to a high degree by fractional distillation after chemical removal of vanadium. The pure chloride is a colourless, limpid liquid.

At the present time rutile is generally employed for manufacture of titanium chloride, since with ilmenite nearly half of the chlorine is converted to ferric chloride, which is of no value.

The Reduction Stage

Magnesium Reduction

We must now deal with the magnesium reduction process.

Titanium, where pure, is a tough and ductile metal, but, as indicated, when contamined by quite small quantities of oxygen, nitrogen, hydrogen or carbon, it becomes brittle and of little use. Thus, though it was prepared as long ago as 1896 in a comparatively pure form, it was not until 1925 that it was made sufficiently pure for its ductility and usefulness to be realised. The method of preparation then used by Van Arkel, consisting in the dissociation of titanium tetra-iodide on a hot titanium filament, was too expensive to be of commercial importance, and it was left to W. J. Kroll to devise a relatively low-cost process about 1938. Until 1955 it was the only process of commercial significance.

The basis of the process is the reduction of titanium tetrachloride by means of molten magnesium, at about 900° C., the primary product being a mixture of magnesium chloride and spongy titanium, together with a considerable amount of unchanged magnesium. Since titanium combines readily with all of the common gases, the reaction must be carried out under helium or argon gas. Usually the magnesium and magnesium chloride are removed by distillation at about 800° C. under high vacuum. This yields titanium sponge, which after crushing to a suitable size is melted to ingots.

The details of the reduction process vary somewhat with different manufacturers, but, generally, cleaned magnesium ingots are placed in a steel reactor. A steady feed of titanium tetrachloride is then begun to the reactor and is continued until about 60 per cent of the total calculated quantity has been added, the temperature inside the reactor being maintained at 800–900° C mainly by the heat of reaction. After tipping off surplus magnesium chloride, the feeding of the titanium tetrachloride is continued. The size of the reactor determines the rate at which heat can be dissipated, and therefore the feed rate of the titanium tetrachloride

When the contents of the reactor have cooled sufficiently the cover is removed, and the reactor is ready to place in the retort. The latter consists essentially of a tallish cylindrical vessel, electrically heated at the top and cooled at the bottom, with connections for evacuating and admitting inert gas. The reactor is located upside down in the upper heated zone, the retort is evacuated to about 5 microns and the heating is applied. The retort assembly and the electrical heating system are contained in an evacuated stout steel casing which takes the stresses due to the atmosphere pressure. Vacuum operation on this scale and at high temperature involves many difficult techniques and problems.

Sodium Reduction

Turning now to reduction by sodium metal.

During the nineteenth century experiments were made on the thermal reduction of titanium tetrachloride by means of metallic sodium. Though the product was

useless, it was shown that the reaction would go, and this subject was taken up again in 1906 by M. A. Hunter in the course of a search for suitable metals for the manufacture of electric lamp filaments. Hunter worked with a bomb of 1 litre capacity filled almost to the top to exclude air—there were no supplies of helium or argon in those days—and from the violent reaction obtained fused globules of metallic titanium. Some of Hunter's titanium was malleable when cold, and it could be readily hot-forged.

The bomb technique does not lend itself to commercial practice, and when in the late 1930's Kroll considered the possible use of sodium for titanium production it was in relation to a reactor working at near atmospheric pressure. More than 80 per cent of the total weight of reaction products consists of sodium chloride, and Kroll concluded that it would be necessary to operate above the melting-point of this material, which is 803° C. Since he also thought that it would be essential to keep below the boiling-point of sodium, 880° C., the apparent temperature range was less than 80° C. He considered this to be too narrow, and it was an important factor leading to his choice of magnesium for the reduction.

However, when, in 1951, Imperial Chemical Industries Limited were searching for a cheaper route to titanium, experiments with sodium reduction were begun mainly because sodium is cheaper than magnesium in this country. Surprisingly it was found that the supposed limitations already mentioned did not in fact cause any great difficulty when titanium tetrachloride was run at a steady rate into a reactor containing hot sodium. Though the sodium chloride product had to be kept molten, its upper temperature limit could be effectively controlled by evaporation of sodium, which then condensed on the relatively cool exposed cover of the reactor, and ran back into the melt. Much of the reaction occurs in the vapour phase above the melt, and owing to the large exothermic heat of reaction, temperatures in this zone can be very high, frequently over 1200° C. In the course of two years the scale of the experimental work grew from a reactor turning out a 7-lb. batch of titanium to one producing 900 lb., and at this stage reactors to make one to two tons per batch were designed for the company's commercial plant at Wilton, which went into production during the first half of 1955. Before the end of the year this plant achieved its rated output of 1,500 tons a year. In America, the first commercial plant employing sodium was erected by the Electro-Metallurgical Corporation and was started up in May of this year.

As in the Kroll process, the reaction is carried out in steel vessels, and afterwards the melt is allowed to solidify and cool before the reactor is opened.

Although sodium chloride can be distilled off from the titanium under vacuum, the operation is slightly more difficult than for magnesium chloride owing to the lower volatility of the sodium salt. On the other hand, aqueous leaching of the primary product is easier. The Kroll melt contains magnesium, which in a leaching process must be dissolved in acid, in the course of which some of the liberated hydrogen is absorbed by the titanium, while at the same time magnesium oxychloride is precipitated in the product by hydrolysis of the magnesium chloride. Difficulties of this type are reduced in the sodium process, and since leaching is basically a cheaper process than vacuum distillation, it appears likely to become the accepted method of treating the product of the sodium reduction stage. One of the advantages associated with leaching is that removal of solidified melt from the reactor and reduction to the desired size range is much easier and safer than the corresponding operations with vacuum-distilled metal. At one time quite a considerable proportion of the Kroll product was lost owing to ignition during these operations. Although the sodium leaching process is not free from complications, procedures have been worked out which unfailingly produce titanium of the highest quality.

Titanium Melting and Fabrication

The consolidation of raw titanium into ingot form is a particularly difficult metallurgical problem, complicated by two factors, namely, the high chemical reactivity of the metal, and its high melting-point (1660° C.). When molten, titanium reduces all conventional refractories, causing failure of the refractory vessel and contamination of the molten metal with subsequent embrittlement of the solid metal. Graphite is the least deleterious refractory for containing molten titanium, but even so, the rate of solution of carbon into the melt is sufficient to give rise to poor mechanical properties. The only suitable type of container for molten titanium is in fact a metal crucible which is water-cooled to such a degree that no reaction occurs between the titanium and the crucible. Copper is almost universally employed as the crucible material, and the only way in which titanium can be kept molten in such a container is by the application of localised intense heat, inside the crucible, as for instance by the electric arc. The operation must be conducted either in vacuo, or under argon or helium. Since 1949, when ingots or 'buttons' weighing only a few ounces were melted, development of the titanium melting furnace has been rapid, and today ingots weighing up to several tons are regularly melted in the U.S.A., although the weight of ingots melted in this country is rather smaller.

The early titanium melting furnaces used water-cooled non-consumable electrodes of tungsten or graphite, but the modern furnace uses an uncooled consumable titanium electrode. The electrode is usually formed by pressing raw titanium, premixed with alloy constituents if required, into compacts, which are welded together to form a consumable electrode of the required dimensions.

At the start of the melt, an arc is struck between the consumable electrode and a small starting charge of raw titanium which is placed at the bottom of the crucible. This initial charge is melted by the intense heat of the arc, and liquid metal flows up to the water-cooled crucible wall, forming a shallow pool of molten metal. The titanium at the bottom of the electrode is also melted in the arc, and drips into the molten pool, and the ingot is built up continuously by this feed of liquid metal. In order to keep a constant arc length, the electrode is fed steadily downwards, and to assist in maintaining a stable arc and to prevent it wandering to the copper crucible wall, with consequent risk of puncturing the latter, it is usual to impose a magnetic field in the melting zone by surrounding it with an electromagnet or solenoid coil.

In this melting process, the whole of the ingot cannot be held molten at any given instant, and the homogeneity of the ingot is thus dependent on the uniformity of composition of the liquid metal falling off the consumable electrode. This is particularly important in the production of titanium alloy ingots, and can be ensured by intimate mixing of the raw titanium granules with alloy constituents, in the form of metal or master alloy granules, prior to pressing into consumable electrode compacts. Normal practice in titanium melting is to double melt all ingots in order to improve the surface condition (and hence the yield) and homogeneity of ingots. The second melting process uses the primary ingot as a consumable electrode, which is remelted in a larger crucible.

Titanium ingots are worked into forgings, sheet, strip, rod, wire or tube by processes which in general are conventional. Titanium fabrication processes differ from conventional methods in two important aspects, both resulting from the extreme reactivity of the metal. One aspect is the danger of contamination, mainly from oxygen, nitrogen and hydrogen, in heat-treating and annealing furnaces, the other concerns lubrication techniques in heavy cold-working processes, such as wire- or tube-drawing. Large ingots are now commonplace, and the titanium melting furnace has grown into a highly developed, fully automatic melting unit,

with a surprisingly high electrical efficiency. Fabrication problems are yielding to investigation, and most wrought forms of titanium and its alloys are now readily available from each of the country's two titanium melters

Since internal soundness is essential for all materials used by the aircraft industry, a rigorous visual and physical inspection is carried out on each ingot and billet, using radiographic and ultrasonic non-destructive examination techniques.

Titanium is no longer a new, exotic metal, but has taken its place as yet another specialised engineering material in a world which is continually increasing its demand for metals and alloys with specialised properties.

NEW ELEMENTARY PARTICLES[1]

by

J. M. VAIL

Endeavour Prize Essay

THE main stream of present-day research in physics is concerned with the phenomena of systems so small in scale that intuition based on everyday experience fails as an adjunct to experimental technique. During the period of time required to develop a new language, an essentially mathematical language, and an intuitive sense connected with it, the language of everyday experience has had to suffice, suitably modified, in thinking about sub-microscopic phenomena. In the imperfect mixture of common and mathematical terms, a concept of 'elementary particle' has been developed, characterised by mass and charge, as inferred from interactions between electromagnetic fields and the particle, and by spin as inferred from a fairly adequate theory and from direct experiment. An elementary particle is generally considered as a concentration of mass and charge, if applicable, at what is effectively a geometrical point, although such an idea is inconsistent with our conventional understanding of what is meant by associating spin with the particle.

Three particles are considered at present to be the stable constituents of all matter: the negative electron, the proton, and the neutron. The term 'elementary' probably developed with the realisation that it is not possible to perform experiments which subdivide these particles into constituents: they are considered not to have more basic constituents. The neutron is not stable for an indefinite period, but decays spontaneously, in a few minutes on the average, into an electron, a proton, and a neutrino. The latter particle is not unstable, but neither is it an important constituent of matter, since it has such a small effect on other particles (1). In recent years a number of unstable elementary particles have been discovered or theoretically hypothesised. Thus the positive electron has its place in the existing theory and is observed, but does not exist in matter for any long period of time, being annihilated with a negative electron in such a way that no mass remains. The mass of the two electrons is converted into electromagnetic energy, in agreement with the mass-energy equivalence concept of special relativity. The same theory which accounts for the positive electron also predicts a negative proton, which has only recently been observed, by Segré and his associates (2). In 1935 a theoretical attempt by Yukawa (3) to explain nuclear forces led to the postulate of another unstable particle: the meson. In 1938, a particle was discovered (4), and although it was at first thought to be Yukawa's meson, it was soon found not to have the strong interaction with nuclei required by the theory. It was named the 'μ-meson'. In 1947 another particle was discovered (5), having the strong nuclear effect required by the Yukawa theory, and it was named the 'π-meson'. Thus far, all the elementary particles known with the exception of the μ-meson fitted into established theories. Later in 1947, however, began a series of discoveries which were not at all to be expected. A number of unstable new elementary particles were observed, with such a variety of properties that attention became focused anew on the concept of elementary particles and on the relationships of these particles to one another.

One of the first of the puzzling new discoveries was made by the Manchester group, observing the cloud-chamber tracks produced by penetrating cosmic-ray

[1] Essay awarded the first prize in the 1956 competition sponsored jointly by the publishers of *Endeavour* and by the British Association. Mr. Vail is a Canadian and is a graduate of the University of Manitoba. He is at present doing post-graduate work at Brandeis University, Massachusetts, U.S.A.

showers. The tracks are vapour trails produced by condensation of the gas in a super-saturated atmosphere on ions produced along the path of a charged particle passing through the chamber. Referring to the original papers (6), one can see photographs of the V-shaped tracks which aroused the scientists' interest. In attempting to interpret the tracks on the basis of well-known interactions (7), such as the scattering of a charged particle by a nucleus, or the creation of a pair of oppositely charged electrons by a γ-ray, insurmountable objections presented themselves. Considering the energies of the charged particles producing the tracks, the following conclusions were reached: One of the tracks must have resulted from the decay of an uncharged particle, since no track leads up to the vertex of the V; The particle must have decayed spontaneously, for the track originates in the gas of the cloud chamber rather than in a dividing lead partition in which the event would have been far more probably due to the greater density, had it been caused by a collision with a nucleus; from the decay products, the parent particle must have had a mass equal to about one thousand electron masses, which is significantly larger than the mass of the π-meson, and less than that of the nucleon (8). The other track was found to represent the decay of a charged particle into one uncharged and one charged particle. Later work (9) by this group at greater altitudes, at which more high-energy cosmic-ray events occur, led them to conclude that there were V-events having as decay products a proton and a π-meson, indicating a parent-particle mass greater than that of the proton. This was indeed a revolutionary result. Conclusions regarding the energy, momentum, and charge sign of the particle producing a track are drawn from the thickness of the track, its curvature, and its direction of curvature in a magnetic field. A particle of high energy will produce a lighter track than the same particle at lower energy, for it will collide with fewer gas molecules, and thus produce less ionisation.

Further studies of cosmic-ray tracks in cloud chambers, by the M.I.T. group (10), added to our knowledge of V-particles and contributed still another type of particle. By using a cloud chamber with parallel metal partitions perpendicular to the cosmic-ray beam, it was possible to observe the creation of the V-particle in the metal, as well as its decay in the gas. By analysing photographs taken at various angles for several events, it was concluded that the line of flight of the uncharged V-particle, and those of the charged decay products, lay in a plane. From this the conclusion was fairly well-grounded that there were no neutral decay products undetected in the chamber. The new type of particle which was observed produced mesons on decay, but was found not to decay spontaneously. The experimental evidence of this fact came from a track which was heavier after traversal of each succeeding metal plate, indicating to the experimenters a drop in momentum. The event was called an S-event, since it was produced by the stopping of the particle in the metal plate.

Still another type of particle came to light through the medium of nuclear emulsions. The principle of this technique is similar to that of the cloud chamber, the ionisation produced by the passage of a charged particle through a photographic emulsion obliquely being directly visible when the plate is developed. In 1949 the Bristol group (11) published a detailed analysis of an event which was observed in an emulsion. The paper is interesting, not only for the discovery which it reports, but, to one not familiar with nuclear emulsion studies, as an example of the amount of information obtainable from such a photograph, and of the reasoning employed in extracting this information. The conclusion drawn from an analysis of the event is that a heavy charged particle of about one thousand electron masses decays into three charged π-mesons. Subsequent observations by other workers support this conclusion.

The term 'curious particles', which was often applied to the new discoveries, indicates to some extent the position which they assumed on their entry into

phenomenology. The existence of the new particles had not been suspected, and their discovery greatly complicated the picture of elementary particles. Their masses at first were thought to have a wide range of values falling into two sets, one set having masses between that of the π-meson and the nucleon, and the other set having masses greater than that of the nucleon, itself a curious property. The relationship of these unstable particles to the common constituents of matter became an increasingly pertinent and difficult question. Following the initial discoveries an awareness of the particular problems presented caused the emphasis in experimental work to shift somewhat. There was the realisation that the detailed physical properties of the new particles must now be determined. The theoretical questions of spin and statistics associated with each new particle required the discovery of the exact nature of decay products. More accurate mass determinations were necessary to ascertain whether the new particles discovered by different groups were different beyond experimental error or not. Measurements of average lifetimes before decay would provide one of the main links with theoretical attempts at explanation. The modes whereby the particles were produced, with their corresponding probabilities, were also points of great interest. The investigation of these detailed properties required two advances: an increase in the accuracy of the experimental measurements, and an increased number of 'curious particle' events to be analysed. The latter point, that of improved statistics, was provided for in part by the new high-energy particle accelerator, the cosmotron, which allowed more numerous controlled experiments than did the cosmic-ray work. In particular, a systematic study of creation processes was possible, along with more accurate determination of the energies involved. The analysis of events was still based on cloud-chamber or nuclear-emulsion tracks. Increased activity in cosmic-ray research, specifically focused on the new particles, further contributed to improved statistics. One of the special contributions of the cosmic-ray studies is that they involve events of far higher energies than are obtained from artificial sources. The advancement of our knowledge of the new particles has also been quickened by the admirable growth of scientific liaison since the war. The proceedings of the various international conferences on cosmic-rays, high-energy physics, and elementary particles constitute the latest compilation of experimental and theoretical results in this field, and help to eliminate repetitious experiments and to emphasise the most fruitful lines of investigation.

Typical of the cosmotron experiments is one reported by the Brookhaven scientists (12), in which two V-tracks were produced together. Protons in a magnetic field are accelerated along a circular path by an oscillating electric field, and then stopped by a metal target. The collision between a proton and a target nucleus at the high energies used results in the creation of π-mesons of about 1·5 billion electron volts kinetic energy. This may be compared with the energy equivalent to the rest mass of an electron, which is a half-million electron volts. Most convincing photographs have been obtained (12) showing the track of a charged π-meson ending abruptly in a collision with a hydrogen nucleus in a cloud chamber, with two V-shaped tracks appearing farther on in the gas. Analysis of the V-events indicated that one was from the decay of a particle into a proton and a π-meson, and the other was from a heavy meson decaying into two π-mesons. Since the end-point of the incident π-meson track lay in the plane of each V-track, and the incident track lay in a plane with the two V-vertices, it was concluded that the π-meson-nucleon collision resulted in the production of both V-particles, and of nothing else. Similar later experiments have contributed much of what is known about the creation processes for the new particles.

Two examples of the interaction of new particles with matter have been discovered. In an emulsion, nuclear explosions caused by direct hits with very high-energy cosmic-ray particles produce star-shaped tracks. In several cases one of the

fragments, a light nucleus judging from the thickness of the track, recoils a short distance and then emits a π-meson. The phenomenon is interpreted by assuming that a particle more massive than a nucleon is produced by the cosmic-ray collision and is bound in the recoiling fragment nucleus, finally decaying into a proton,

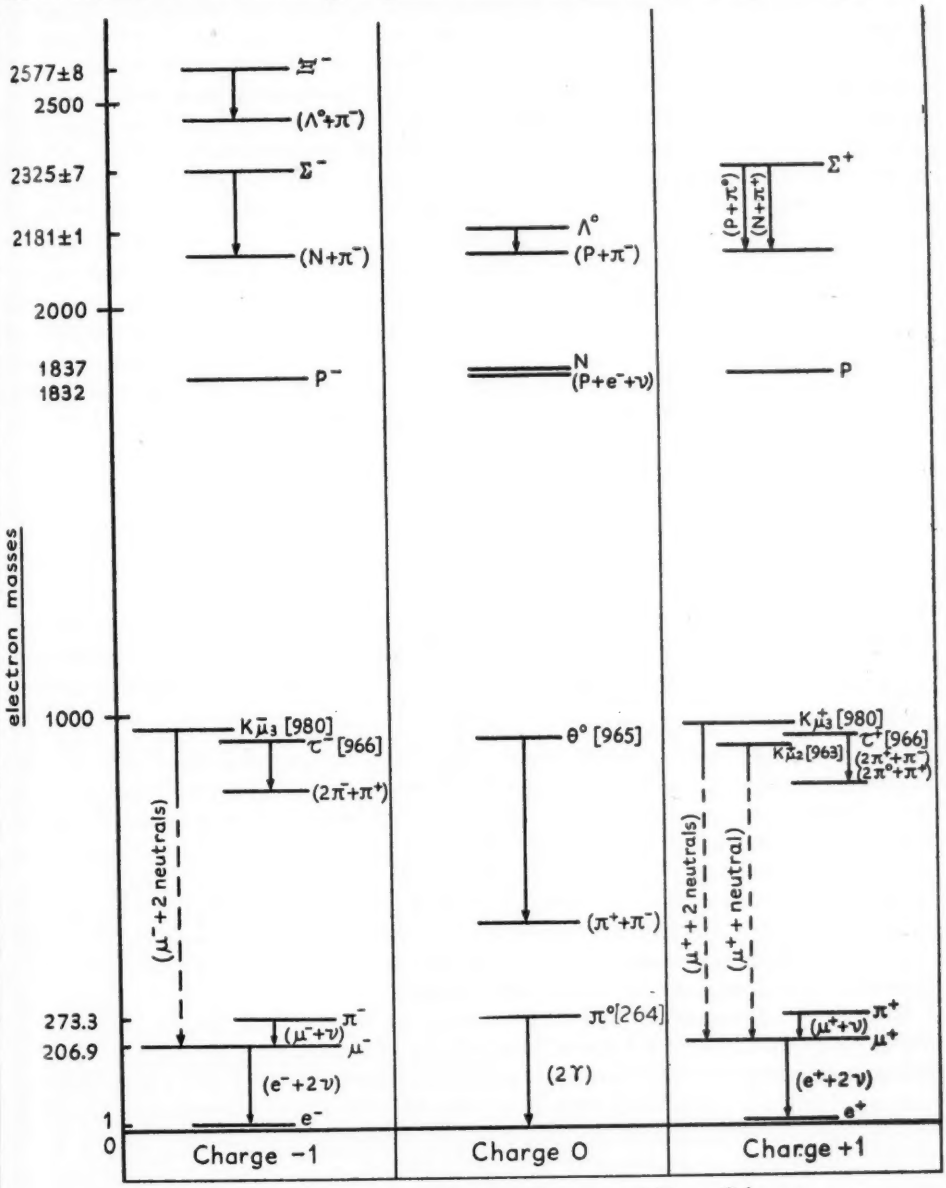

Diagram of Elementary Particle Masses, Charges, and Decay Schemes.

which remains bound, and a π-meson, whose emission is observed. This is a very important observation in attempting to understand the new particles, for it indicates that some at least interact with other particles in a similar way to nucleons, to the extent of occupying a place in a nucleus. Another frequently observed phenomenon is the capture of negatively charged heavy mesons by nuclei in emulsions, producing a star which indicates a large energy release. This, then, is a further indication of a strong interaction between nucleons and new particles.

As the experimental work progressed into its second stage it became possible to tabulate certain properties of the new particles and a fairly uniform convention in terminology was adopted. Particles heavier than the nucleon are called 'hyperons', and are denoted by Y if unidentified, and by capital Greek letters if known to be of a certain type, with superscripts indicating the charge and subscripts indicating decay products, if the latter are not all identified. Heavy mesons of mass intermediate between those of the π-meson and nucleon are denoted by the letter K, with appropriate subscripts and superscripts if unidentified, and by lower-case Greek letters if identified. The accompanying diagram shows the best verified general properties of the new particles, and for the sake of completeness, of the other elementary particles also. The vertical scale is in electron masses and vertical arrows represent decay schemes, with the products in curved brackets. The quantities in square brackets beside the heavy mesons $K^{\pm}_{\mu 3}$, $K^{+}_{\mu 2}$, $\tau^{\pm}$, θ°, are their respective masses. The symbols N, P, and e represent neutron, proton, and electron respectively, while ν and γ represent neutrino and γ-ray respectively. The three columns correspond to different charges as indicated. The production processes for the new particles have been determined by using cosmotron-produced π-mesons or the direct proton beam on gaseous media in a cloud chamber, as was done by the Brookhaven group. The processes may be symbolically written:

$$\pi^{-} + P \longrightarrow \Sigma^{-} + K^{+}.$$

$$\pi^{-} + P \longrightarrow \Lambda^{\circ} + \theta^{\circ}.$$

$$P + P \longrightarrow \Lambda^{\circ} + K^{+} + P.$$

The τ-meson three-π decay process is that described by the Bristol group (11) using emulsions. The θ° and Λ° particle decays give rise to the V-events observed originally by the Manchester group (9). The Ξ^{-} particle decay, which is called a cascade because of the subsequent Λ-decay, was observed in cosmic-rays. Besides the $K^{\pm}_{\mu 3}$ and $K^{+}_{\mu 2}$ particles shown in the diagram, there is a $K^{\pm}_{\pi 2}$ which was omitted to prevent crowding the figure. Its mass has been determined to be $962 \pm 1{\cdot}6$ electron masses and besides a π-meson, it has one uncharged decay product. Since the identification of decay products relies on the ionising ability of the particles, neutral particles cannot be easily detected. Neutrinos cannot be observed at all because of their extremely weak nuclear interaction, and γ-rays in nuclear plates and cloud chambers are not easily detected under the conditions used in observing these events. For the same reason a π°-meson would be difficult to detect from its γ-rays, were it a neutral product of a new particle decay.

When the experimental data had reached the point where details like those in the diagram could be relied upon, several facts of theoretical importance emerged. Since the electron and nucleons have spin $\frac{1}{2}$, and obey Fermi-Dirac statistics quantum mechanically, and since spin is conserved, while the π-meson has zero spin, and is therefore classified as obeying Bose-Einstein statistics, it was inferred from:

$$\Lambda^{\circ} \longrightarrow P^{+} + \pi^{-}$$

that the Λ° had spin $\frac{1}{2}$, and was therefore a Fermion: a particle obeying Fermi-Dirac statistics. Similarly, the $\Sigma^{\pm}$ particle is a Fermion, and so is the Ξ^{-}. Thus we see that all particles of mass greater than, or equal to, the nucleon mass are Fermions. On the other hand, the τ and θ particles are Bosons, for the sum of the spins of their decay products is zero: they obey Bose-Einstein statistics. Another point of interest in the diagram is the fact that every hyperon decay scheme results in a particle of nucleon mass or greater. Particles of such mass are sometimes called

'baryons', and we can see that there appears to be a principle of conservation of baryons. These were some of the basic facts with which any proposed theory of elementary particles would have to agree.

Probably the most puzzling feature about the new particles was the average time between production and decay, which ranged between 10^{-10} seconds and 10^{-8} seconds. From this mean lifetime it was possible to make a theoretical estimate of the strength of the interaction causing decay. It was possible also to estimate the strength of the creation interaction, for when a target was bombarded by cosmotron protons, several per cent of the secondary particles produced were new unstable particles, the rest being π-mesons. The new particles must therefore interact with nucleons to about the same extent as does the π-meson. An estimate based on such reasoning disagreed badly with the estimate based on mean lifetimes for decay. The existing theory for interactions required that decay and production should occur with about the same ease, whereas the lifetime was too long, the decay process too weak, to give agreement. This discrepancy, inconsistent as it was with the existing field theory of elementary particles, provided the first clue to explaining the behaviour of hyperons.

Two attempts to explain the discrepancy have been made. The first, based on the model of a hyperon as a composite particle of nucleon and π-meson, is not popular because it requires angular momenta much greater than those which occur in similar bound systems, such as light nuclei. The second attempt postulates that the production of the new particles occurs in such a way that one hyperon and one heavy meson occur together, never singly. This is called 'associated production'. The particles are observed to decay singly. Then one concludes that the associated production interaction is a strong one, since the particles are produced copiously, and that the single decay modes are the result of relatively weak interactions, since they occur relatively slowly. By a strong interaction is meant one of similar strength to that existing between nucleons. Electromagnetic interactions are known to be weaker, while the new particle decay interaction is assumed to be still weaker, possibly comparable to that mediating β-decay. Experimental evidence, while not extensive enough to allow a final decision, does give examples of associated production, and no definitely contrary examples thus far.

The concept of associated production amounts theoretically to a selection rule for strong interactions, and has suggested to a number of theorists the idea of associating quantum numbers with the various elementary particles. In quantum mechanics the quantum numbers and their selection rules refer to the possible values and changes of dynamical variables, most of which have classical counterparts, such as energy or angular momentum. Such dynamical variables relate to the properties of a physical system's motion in the geometrical space of common experience, and in time. The special theory of relativity considers an abstract space of four dimensions containing the three spatial dimensions and time. In a similar way the idea arose of relating the variables connected with the quantum numbers for strong interactions to a space, which must be abstract.

In examining what is known of strong interactions such as the nucleon–nucleon interaction, one is struck by the fact that the interaction is independent of the charges on the nucleons: between two neutrons the force is the same as between a neutron and a proton. This is an invariance under changes of the charge, which for nucleons has two possible values, 1 or 0. Now spin-$\frac{1}{2}$ particles are observed to have two possible spin values: $\pm\frac{1}{2}$. Spin for such particles had been described by a vector whose component in a given direction could have only these two values depending on the orientation of the vector in space. By direct analogy a spin type of vector called 'isotopic spin' was introduced, related to an abstract three space, in order to describe the two charge states of a nucleon. If I_3 is the component of the isotopic spin vector in a given direction, then I_3 is $\pm\frac{1}{2}$. If we let $I_3 = +\frac{1}{2}$

represent the proton state, and $I_3 = -\frac{1}{2}$ the neutron, then charge Q may be defined by:

$$Q = I_3 + \tfrac{1}{2}.$$

Charge invariance of a two-nucleon system then meant that the theory must be independent of the orientation of the isotopic spin vector in its space, which is equivalent to saying that nucleon–nucleon interactions are invariant under rotations of the isotopic spin space axes. The approximately equal strength which had been noticed for the production interactions for the new particles and nucleon–nucleon interactions then implied that these new particle interactions had the same invariance property under isotopic space rotations. The hope was to find a variable function in isotopic space whose possible quantum mechanical values would allow a classification of elementary particles.

Two types of approach to the problem were made. One was a semi-empirical approach in which selection rules were assumed for a parameter, and values of the parameter were assigned to each particle in such a way that observed interactions were predicted by the selection rules. Two reasonably successful schemes of this type have been advanced, one by Gell-Mann (13), the other by Sachs (14). There is considerable justification for a semi-empirical approach, for the present field theory is generally conceded not to be satisfactory, and an attempt to extend an unsatisfactory framework farther into the unknown will produce much that is not of lasting value. On the other hand, field theory does give many results which are qualitatively valid, and an extension might show where modifications are necessary. An early theoretical study by Pais (15), considered the behaviour of interactions under reflection of axes, or reversal of co-ordinates, in isotopic spin space, but this did not give sufficient new results to cope with the classification of new particles. Pais then introduced (16) a four-dimensional isotopic spin space, which allowed prediction of events which, although they should be observable, are not observed. While the importance of isotopic spin space in the theory of new particles was fully realised, no large theoretical success had occurred prior to 1955, in obtaining general qualitative agreement with experiment.

More recently an idea of D'Espagnat and Prentki has succeeded in classifying the heavy particles and shows promise of producing something new in understanding the rôle of isotopic spin in the new particle interactions. In the ordinary four-space of special relativity, Fermions are described by spinors, which are mathematical forms distinguished from others by the change produced in them by a rotation of the reference axes of the space. Spinors are defined for a three-space also, and fall into two classes which differ only in their behaviour under reflection of the three-space axes. There are other quantities called scalars, pseudoscalars, vectors, and pseudovectors, which are likewise distinguished by their behaviour under rotations and reflections of axes. The following table lists the number by which each so-called covariant is multiplied when the three-space axes are reversed in direction. This number is called the 'parity of the covariant quantity'.

Covariant	*Parity*
scalar	$+1$
pseudoscalar	-1
vector	-1
pseudovector	$+1$
spinor of the first kind	i
spinor of the second kind	$-i$

The number i is defined by $i = +\sqrt{-1}$. The work of D'Espagnat and Prentki (17) was based on two hypotheses: the conservation of heavy particles, and invariance of strong interactions under reflections of isotopic spin space co-ordinates, as well as under rotations. They then showed that if one considered only particles described in isotopic space co-ordinates by scalars, pseudovectors, or either kind of spinors, as well as by ordinary space and time co-ordinates, the existing theory of elementary particles led to a quantity which they called U, which allowed a classification of particles equivalent to the phenomenological scheme of Gell-Mann (13). Later work by Racah (18) indicated that for a particle described by a covariant having parity p, the value of U could be determined from the equation

$$p = i^U,$$

which can be written (19)

$$p = e^{iU\frac{\pi}{2}},$$

where e is the base of natural logarithms (2·71828 . . .) and π is the ratio of circumference to diameter of a circle (3·14159 . . .). The earlier work (17) had shown that charge was definable in general for elementary particles by

$$Q = I_3 + \frac{U}{2}.$$

It was known that to multiply a covariant by $e^{-iI_3\alpha}$ has the same effect as that produced by a rotation of axes by α-radians about the 3-axis, I_3 being the component of I parallel to this axis. Then $e^{iI_3\pi}$ represents a rotation of π-radians, or one-half of a revolution about the 3-axis. Then since p, the effect of reflection of axes, was represented by $p = e^{iU(\pi/2)}$ it followed (20) that

$$e^{iQ\pi} = (e^{iU(\pi/2)})\,(e^{iI_3\pi})$$

represents a half-revolution about the 3-axis, and then a reflection of all three axes through the origin. This reflection replaces the 1- and 2-axes to their original positions and reflects the 3-axis: that is, $e^{iQ\pi}$ represents a reflection of the 3-axis in the 1–2 plane. It therefore appears that the quantum mechanical charge operator generates reflections in the 1–2 plane in isotopic spin space. A connection has therefore been established between the definition of charge and the mathematical framework of quantum theory. Whether such a definition will lead to a better understanding of elementary particle interactions has yet to be investigated.

We have seen in the story of the new particles a rather lucid example of the evolution of scientific knowledge. Early in 1947, the theory of elementary particles appeared to be qualitatively well-grounded. The new discoveries of the ensuing six years increasingly upset this relatively simple situation, and through the copious production and long lifetime anomaly, posed a problem of central importance. Concentration on this 'inexplicable' point of behaviour then led to the apparently satisfactory associated production hypothesis. Study of the ramifications of such an hypothesis produced the current hopeful avenue of approach toward more thorough understanding of elementary particles, through their isotopic spin space properties. Out of the very climax of the difficulties has arisen the apparent key to a solution.

Recently, extensive experiments undertaken at Berkeley and at Brookhaven (21) have determined a number of important properties of heavy unstable particles. These experiments use for the detecting device a bubble chamber (22) which is similar in principle to the cloud chamber, but uses liquid in place of vapour. The greater density increases the chance that a decay-product particle will be stopped inside the chamber, and that full analysis of the event will therefore be possible. At Brookhaven, the Σ° has definitely been identified, through its decay

$$\Sigma^\circ \longrightarrow \Lambda^\circ + \nu,$$

which is fast: of the order of 10^{-20} seconds. Further recent data indicate, within an experimental error of one or two electron masses, that the masses of *all* heavy mesons are the same. Experiments are in progress (23) to test whether the θ° is a 'particle mixture'. For a discussion of the concept of a 'particle mixture', which is beyond the scope of this essay, the reader should refer to the original paper by Pais and Gell-Mann (24).

At Berkeley (25), bevatron-produced K mesons have been used to study Σ hyperons produced by the reaction

$$K^- + P \longrightarrow \Sigma^\pm + \pi^\mp$$

The Σ^+ and Σ^- are found to have significantly different average lifetimes: $1{\cdot}9 \times 10^{-10}$ seconds and $0{\cdot}9 \times 10^{-10}$ seconds respectively, with probable error of the order of twenty per cent. Angular correlation studies of the decay products of Σ particles indicate that the Σ particle probably has a spin of $\frac{3}{2}$ rather than $\frac{1}{2}$. If this last result is corroborated, it will help the new particles to maintain their reputation of being 'curious'.

The author wishes to thank Prof. H. R. Coish, who first interested him in this subject, and Prof. M. Chrétien, who acquainted him with the most recent experiments in the field.

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(4) Anderson & Neddermeyer (1937): *Phys. Rev.* **51,** 1005.
(5) Lattes *et al.* (1947): *Nature, Lond.* **160,** 453.
(6) Rochester & Butler (1947): *Nature, Lond.* **160,** 855.
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(17) D'Espagnat & Prentki (1956): *Nuclear Physics*, **1,** 33.
(18) Racah (1956): *Nuclear Physics*, **1,** 302.
(19) D'Espagnat & Prentki (1956): *Phys. Rev.* **102,** 1684.
(20) The equations:
$$e^{iQ\pi} = e^{i[I_3 + (U/2)]\pi} = e^{[iI_3\pi + iU(\pi/2)]} = (e^{iI_3\pi}) \cdot (e^{iU(\pi/2)})$$
will be self-evident to a reader with some mathematical knowledge.
(21) Some of the results quoted have not yet been published. For preliminary results from Brookhaven, see Budde *et al.* (1956): *Phys. Rev.* **103,** 1827.
(22) For detailed description, see Glaser & Rahm (1956): *Phys. Rev.* **97,** 474, and Brown, Glaser & Perl (1956): *Phys. Rev.* **102,** 586.
(23) Blumenfield *et al.* (1956): *Phys. Rev.* **102,** 1184. Landé *et al.* (1956): *Phys. Rev.* **103,** 1901
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THE SOCIAL EFFECTIVENESS OF THE MENTALLY HANDICAPPED[1]

by

DR. N. O'CONNOR

Two points stand out clearly from the results of recent research with defectives. The first is that they are handicapped in the performance of tasks which involve the use of symbols, and therefore abstraction. The second is that although one would expect that such a handicap would also be reflected in performance on motor tasks, this is not necessarily the case. There is a relationship between intelligence and performance, but it is not so close that great individual variations do not take place. In fact, the ability of both the feeble-minded and imbecile patients to carry out simple repetitive tasks is much greater than their I.Q. would lead us to suppose.

The work reported here was carried out with defectives whose I.Q. averaged 33, i.e. about one-third of normal. However, previous research suggests that most of the results are also true of the feeble-minded whose I.Qs should range from 50 points to 70 points, i.e. about one-half to two-thirds of normal.

Now if, in fact, intelligence level does not alone determine the output and performance of defectives, other factors may be presumed to be operating. The first question asked by the Social Psychiatry Research Unit, therefore, was, 'What conditions of the environment maximise the output of imbeciles?' The investigation began with a study of incentives.

Dr. Gordon, in showing the effects of incentives on the performance of imbecile patients, elicited the information that even those imbeciles who have been living in hospital for a number of years without any specialised attention other than that normally given to chronic patients, will respond very satisfactorily to incentives. The task given to the patients in this case was to insert small escutcheon pins in holes in a perforated metal sheet. This was carried out for an hour each day by male patients. On first examination it seemed that there was no difference between first and second half-hours in the trials. Later a difference was shown to exist. In the case of patients working under incentive conditions a red-headed pin was inserted to mark a point slightly above the previous best performance. Patients showed considerable improvement if asked to work up to the level of a goal determined by their own previous attainments. The material presented in Fig. 1 shows the effect of setting goals for a group of imbecile patients as compared with a matched group who had not the benefit of this form of incentive. The differences in performance are statistically significant, and have also considerable theoretical importance.

When this work was completed in 1953, it was decided to continue a similar experiment with patients of the same kind, but to attempt to make some theoretical advances. For example, it was not known whether the effects of improvement in performance would be permanent or temporary, nor was it known whether the effects of improvement might not be reduced if the particular incentive conditions were terminated. Further work was therefore carried out by Dr. Claridge, who tested the performance of the same groups one year after they had completed the training indicated in Fig. 1. The lettered lines on Fig. 1 indicate the levels to which performance had been reduced in the interval. It will be noticed that the effects of the

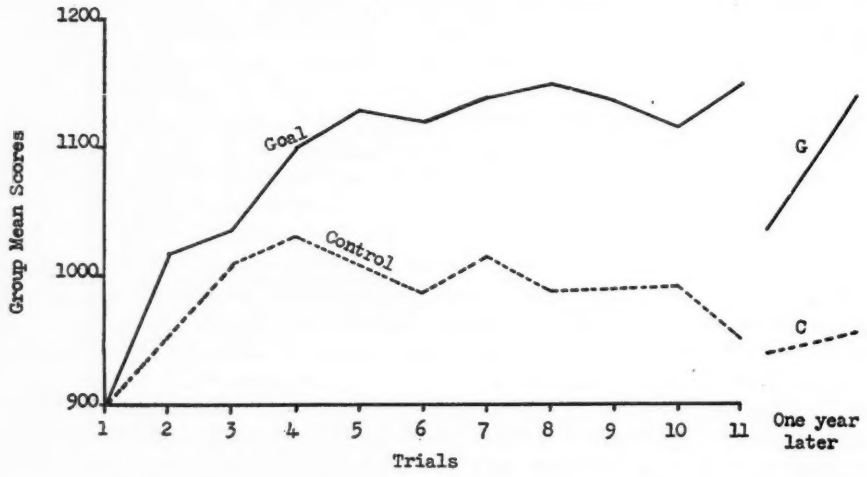
[1] Paper given to Section J (Psychology) on August 30, 1956, at the Sheffield Meeting of the British Association. The work reported was carried out for the Medical Research Council Social Psychiatry Research Unit. See also *The Social Problem of Mental Deficiency*, by N. O'Connor and J. Tizard, Pergamon Press, November, 1956.

incentive are apparent even after this period. In other words, the original effect of the incentive is maintained.

Dr. Claridge next carried out a further experiment to compare the incentive effects of self-competition alone, that is to say, attempting to exceed previous levels, and self-competition plus the social recognition of successful effort. The hypothesis was advanced that the imbeciles were responding to personal encouragement, or, in other words, in this context, the recognition of achievement by the supervisor or experimenter. His results showed that encouragement and the recognition of achievement were just as important, if not more so, than the simple

FIGURE 1

Comparison of Performance of Incentive and Non-Incentive Groups

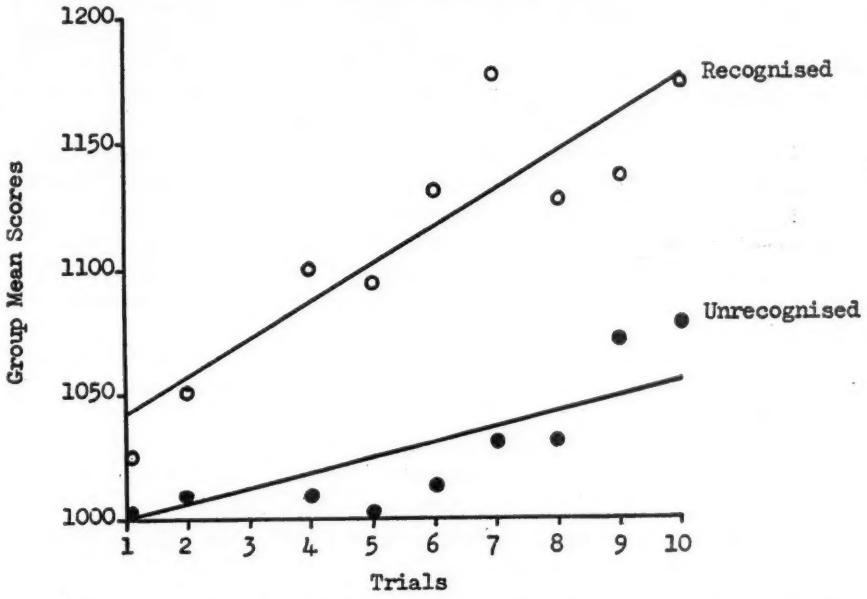


achievement of a goal based on previous levels. Fig. 2, based on regression lines depicting improvement, makes this point clear. The topmost line shows the improvement resulting when self-competition is recognised, and the lower line shows the lesser improvement resulting from successful self-competition which the experimenter ignores.

It was thus clear at this stage of the investigation that defectives, even of a grossly handicapped kind, who had been separated from a normal social environment for some years, were none the less capable of performance improvements of a high order, when appropriately stimulated. As a matter of theoretical interest, an attempt was made to see whether their changes in performance due to the operation of incentives would result in a rise which would be greater if the incentive were introduced early in learning, or greater if introduced later on in learning. The learning theory involved is technical, and is not presented because of space restrictions, but it is enough to say that this particular experiment failed in its purpose because an effect hitherto unsuspected was noticed. Female imbeciles whom we used in this experiment did not improve in the same way as men, and working for half an hour instead of for an hour did not result in the same sort of improvement that had been expected from previous results with one-hour trials. These accidental findings led to a re-analysis of results. When difficulties had been overcome, the experiment was repeated under more satisfactory conditions, and it was discovered that the question of how an early incentive affected learning as compared with a later incentive could be answered briefly as follows. If a group which has been given the best incentive conditions from the beginning is compared with the group which begins without the addition of this incentive, the former group is seen to

FIGURE 2

Recognised and Unrecognised Achievement

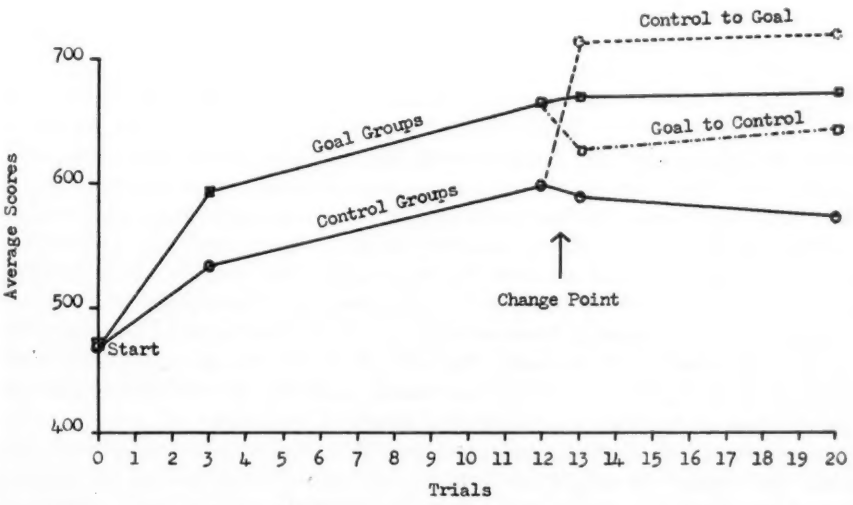


move rapidly to a higher level. The results for two matched groups are presented in Fig. 3.

In what follows, a change in slope means a change in the increment or gain from

FIGURE 3

The Effect of Incentive Early and Later in a Learning Curve



trial to trial so that on a graph the gradient of the line is seen to change. A change in level is a change from one point for one trial to a much higher or lower point for the next. In such a change instead of a change in gradient there is a sharp break.

It will be seen that the improvement of the Goal Group, although notable, does

not greatly exceed that of the other group, in the first introduction of a goal. However, when an incentive is introduced at a later point in the learning process, the change in performance level of the group which originally began under poor conditions is so great and immediate that it exceeds in level the performance of the group which for some time had been operating under the best incentive conditions. This is the group marked 'Control to Goal' in Fig. 3. Although the level of performance changes sharply subsequent to the introduction of an incentive, none the less, the rate of change of performance or the improvement rate shown by the slope of the lines in Fig. 3 does not change, and seems to be rather more a characteristic of the individual. The remaining lines in Fig. 3 show the results of terminating incentives. These are obviously less dramatic than the results due to the introduction of incentives, but are none the less important. This is the line marked 'Goal to Control'. It shows, in fact, that a change for the worse in incentive conditions does not necessarily result in a significant fall in performance level. The result simply confirms that mentioned earlier.

This is the general picture, but there are a number of specific conclusions not accounted for by changes of this sort. To understand them we must look at individual differences.

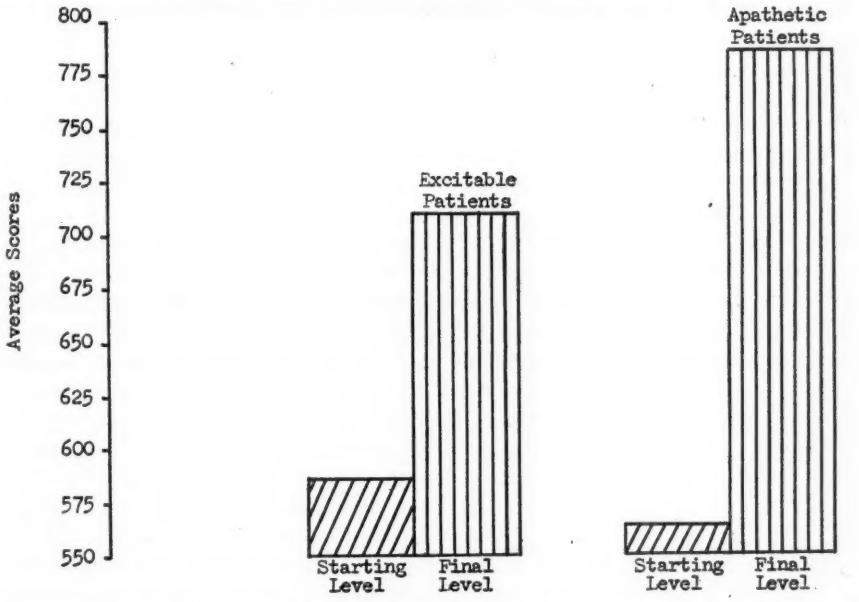
The well-known expert in mental deficiency, A. F. Tredgold, as a result of his clinical experience, drew a distinction between inert and excitable defectives. In this respect he was simply expressing in another way ideas which run through psychiatry from Greek times onwards, and are reflected in the physiology of Pavlov with his concept of excitation and inhibition and with his divisions into types of nervous system. When a distinction was made between relatively excitable and relatively unexcitable defectives, we found that their response to changes in incentive conditions differed notably. This was done by arranging all the imbecile subjects examined on a scale of excitability, as judged or rated on their social and verbal behaviour. It was thus possible to grade them from lively and responsive to stimuli, to inert and unresponsive. The response of the two groups to incentives could thus be compared. Without incentive the excitable type of male defective improves less than the less excitable (or inhibitable) type, particularly on long trials on repetitive tasks. But with incentive, he improves more than this other group. Conversely, the inhibitable type without incentive improves significantly more than the excitable group, and with incentive, improves slightly less. The diagram, Fig. 4, shows the difference between excitable and apathetic imbeciles at the beginning and end of a series of trials, where, although starting on roughly the same level, the apathetic defectives improve considerably more, roughly 36 per cent, compared with 24 per cent for the excitable. By taking Subjects from each extreme of the distribution, percentages become 44 and 20 respectively. The differences between men and women have not been so clearly worked out, and need further investigation, but although men and women do not differ greatly in excitability, women seem to set themselves a rate of work roughly equivalent to that adopted by men under the best incentive conditions. Thus, when encouraged by good incentives, instead of improving, they fall off in performance in some cases, a phenomenon not unlike that noticed by other workers in the field of reaction time study, and which might be compared with Pavlov's paradoxical phenomenon, i.e. when increasing intensity of stimulus leads to a fall off rather than an increase in strength or speed of response.

The upshot of this work has been to show that even with the supposedly simple problem that imbeciles might be thought to present, there exists an unexpected degree of complexity in their psychology. For example, their level of performance is subject to considerable improvement, and in some cases can be shown to overlap with that of normal workers on simple tasks. Contrary to expectations with patients of this intelligence level, they are responsive to social pressures which have a profound effect on their performance. Thirdly, their personality differences seem to be

as considerable as those to be expected among normal people, and have a like effect on their behaviour and response to changes in external conditions. As with some American studies with normal people, a difference between men and women in this response is also found, even at this relatively low level of performance or intelligence. The most important conclusion, however, is that with defectives, even with imbeciles who are normally described as 'persons incapable of managing themselves or their affairs or . . . being taught to do so', there exists the possibility of training them to an unexpectedly high level of performance, of evoking in them a considerable degree of self-involvement, and of leaving with them a lasting improvement of performance as a result of this training technique. It is perhaps not too much to claim that this work, which, whilst novel in the twentieth century, was foreshadowed by the works of the great educators in the eighteenth and nineteenth centuries in Spain and France, may be laying the foundation of an entirely different

FIGURE 4.

Effect of Control Conditions on Excitable and Apathetic Patients



method of the treatment of imbeciles than that which has satisfied us in Great Britain for the last four decades.

This work is the first step in a study of imbecile performance. It has demonstrated that imbeciles, no less than the feeble-minded, have suffered too much from the recognition of their poor level of intelligence. Perhaps the tendency to make more use of the potentialities of the handicapped can be seen a little more clearly if we contrast two social problems: that of selection at eleven, and that of the employment of the ageing. In the first case, we have had too few schools and jobs, and selection is thought to be necessary. In the second, the increasing age of the population has turned the attention of research workers to finding work for those who would previously have been debarred from it by reason of their age. It is not so long since the problems of ageing were ignored. Now we pay them great attention. If the need for technicians proceeds, we may find that the difficulties of educating the less gifted in our secondary schools may have to be overcome. It seems very likely that we will be able to solve this problem now that it begins to present itself as a problem. The same may be true of the problem of educating the backward. Advances will be made as soon as we begin to pose the question.

VERY HIGH TEMPERATURE REACTIONS[1]

by

PROFESSOR G. PORTER

I HAD better begin this paper on very high temperature chemical reactions by saying what I mean by a very high temperature. The present range of chemical technology extends to about 2000° C. for most purposes, and to rather higher temperatures in special cases such as the extraction and refining of very high melting-point metals. If we go to still higher temperatures we enter a very different world—a world where solids and liquids no longer exist and where the gases are composed mainly of simple molecules containing only two or three atoms. It is this world which I should like to talk about. Although no solids can stand up to them, such high temperatures can be readily produced—for example, by the electric arc and by chemical reactions in gases—and their application is being increasingly sought in chemical synthesis, such as the fixation of atmospheric nitrogen, and particularly for the production of power using the principles of the jet engine and that old Chinese invention—the rocket.

When man discovered fire, perhaps about a quarter of a million years ago, he gave himself a source of high temperatures which has been exceeded only relatively recently. His first experiments in high temperature chemistry were undoubtedly in the thermal breakdown of foodstuffs which he used to assist the lower temperature degradative chemistry of his stomach. Later he learned how to utilise the intense heat of the combustion process to make for himself a whole range of new and useful materials of which metals were and still are of first importance. Still more recently he made another discovery of supreme importance when he found that he could convert this heat into energy by means of an engine and make it do his work for him. The study of the theory of the heat engine revealed a universal principle which is at the bottom of our present efforts to produce and utilise still higher temperatures. This principle is a consequence of the second law of thermodynamics, and it says that if we wish to make our engine work at the highest possible efficiency then we must operate it at the highest possible temperature. This is as true of the more exotic jet engines and rockets as it is of steam and internal-combustion engines. Progress in high speed flight and in many other fields depends to a large extent on our learning more about the nature and mechanism of reactions at very high temperatures. Before we consider some of the interesting theoretical questions which arise in this connection and the laboratory methods which are being developed for their study, let us look briefly at some of the methods of producing high temperatures by means of chemical reactions, that is, of converting chemical energy into high temperature heat.

The ordinary bunsen-burner flame attains a temperature, just above the reaction zone, of 1800° C., whilst an acetylene–air flame reaches 2300° C. These are quite near to the temperatures which we should arrive at if we calculated the temperature quite simply from the known heat of the chemical reaction and the thermal capacity of the burned gases. If we exclude nitrogen and burn our fuel in oxygen, we get higher temperatures still—for example, a stoichiometric methane–oxygen flame reaches 2700° C. Now, however, the calculated temperature is much

[1] Paper read before Section B (Chemistry) on August 30, 1956, at the Sheffield Meeting of the British Association.

higher, 5000° C. in this case, since the products above the reaction zone are no longer the simple ones of water and carbon dioxide but contain also hydrogen and oxygen atoms and free radicals in high concentration. For example, in this methane flame the free radical OH forms 14 per cent of the total products in the burned gases just above the reaction zone. The search for still higher temperatures from chemical reactions is therefore not simply a search for reactions which give out more heat, but for reactions which give products which are not highly dissociated even at high temperatures and which therefore utilise their heat of reaction to raise the temperature rather than to dissociate the products of reaction. Hydrogen plus oxygen is quite good in this respect, but hydrogen plus fluorine is much better and reaches 4500° C., hotter than the electric arc. Cyanogen plus oxygen is one of the hottest flames known—4700° C. Powdered metals also give very hot flames. The thermite process using aluminium powder for metal smelting is well known, and the oxyaluminium flame attains a temperature of 3500° C. The beryllium–oxygen flame is much better and reaches 4500° C. These temperatures are high enough to melt any solid known and their full utilisation will depend on methods which do not involve close containment of the burning fuel. Some of the fuels which are at present being used in rocket research certainly make one's hair stand on end. Perhaps you wouldn't mind a trip into space alongside a tank of liquid oxygen or concentrated nitric acid or even liquid fluorine. But how would you like a large tank of liquid ozone as a fellow passenger?

These temperatures probably represent the limit to what can be done with what we might, in a broad-minded sense, call conventional fuels. We could get higher still if we could use atomic fuels—and here I mean truly atomic, oxygen atoms for example, and not nuclear energy. This of course sounds quite out of the question, but it is wise not to be too cautious. There happens to be a source of oxygen atoms just where it is required—in the upper atmosphere. In fact, about eighty miles above the earth the sun's energy maintains a concentration of oxygen atoms higher than that of oxygen molecules, although unfortunately both are at very low concentrations.

What I have said will have shown the importance of more knowledge about the nature and mechanism of chemistry in gases at very high temperatures, and it is time we came down to earth and looked, in a little more detail, at some of the features which distinguish chemistry and research at high temperatures from more ordinary chemistry. The fundamental principles of thermodynamics and chemical kinetics are as valid at high temperatures as at low, but certain new factors become important, and, even apart from these, the relative rates of the various possible reactions may be so changed that the system bears little resemblance to anything happening at lower temperatures. As I have already indicated in the case of high temperature flames, new chemical substances such as free radicals may be major constituents of the reaction, and the study of the chemistry of these substances at high temperatures becomes important. The other factor which introduces the need for new thinking is the extremely rapid rate with which a chemical reaction proceeds at high temperatures. To a close approximation the rate of a chemical reaction can be expressed by the simple equation

$$\text{rate} = A \times 10^{-B/T}$$

where A and B are constants. A is generally a very large number, so that the rate of the reaction is only kept in bounds by the exponential term. As the temperature is raised, this latter term becomes smaller and the rate of the reaction increases very rapidly. For example, in the thermal decomposition of a typical fuel such as ethane the constant A in our equation might be about 10^{14} and B about 15,000, when our rate is measured per second. Using these values, we can calculate the time taken for 50 per cent reaction at various temperatures. We find:

Temperature (° C.)	$t_{\frac{1}{2}}$
20	10^{30} years
300	30,000 years
500	2 weeks
600	5 minutes
700	10 seconds
1000	1 three-hundredth second
1500	1 millionth second.

Now 1500° C. is a quite low temperature for a flame, and therefore if we wish to study the reactions which take place in any of the systems which we have been talking about we must expect to have to work very rapidly; in fact, in general, our reaction is going to be over in times of microseconds (millionths of a second) or even less. Modern electronic techniques have made this sort of measurement quite possible—in fact, it is possible to work with times as small as millimicroseconds, that is one thousand-millionth of a second. But a much greater difficulty arises. Suppose we want to measure the rate of decomposition of ethane at 1500° C., which, as we have seen, is over in about one millionth of a second, how are we to heat the gas up to this temperature? You will remember that the reaction at 1000° C. took only one three-hundredth of a second, so that, if in heating the gas to 1500° we stay at 1000° longer than one three-hundredth of a second, it will nearly all have reacted before we reach the temperature at which we want to begin our experiment. Therefore, if we simply let our ethane into a tube which was heated to 1500° C., we should have no hope of making any useful measurements because the rate of its decomposition would be determined entirely by the rate of conduction of heat from the walls of the vessel, which takes much longer than the time of reaction. This difficulty is so serious that until very recently it has been impossible to obtain information directly about the kinetics of reactions in this range of temperature and we have had to be content with long extrapolations from measurements made at lower temperatures, which, as we shall see, are not at all reliable.

Very recently, however, two methods have been developed which make it possible to do the trick of heating a gas to a very high temperature, in a minute fraction of a second, quite uniformly, without the necessity of heating the vessel at all. The principles of both methods are very simple, and I think you might be interested to hear about them.

The first method uses what is called a shock tube, which can generate extremely violent shock waves. When a gas is suddenly compressed it gains some heat. This principle of adiabatic compression has in fact been used for the study of high temperature reactions, but has many limitations. When the pressure is applied with extreme speed, such as is generated in an explosion or by an aeroplane flying faster than sound, a far higher proportion of the mechanical energy—in fact, most of it—is converted into heat. At Mach 4, i.e. four times the speed of sound, the nose of an aeroplane would be heated to nearly 1000° C. if it were not cooled, and at Mach 10 the shock wave heats air to 3000° C. Here is another reason why high speed flight is demanding more information about high temperature chemical reactions. If the aerodynamics of missiles and vehicles flying at these speeds is to be properly understood, it will be necessary to make allowance for the fact that chemical reactions are taking place in the shock front ahead of the aeroplane or rocket and changing the chemical composition of the air.

In the laboratory the shock wave can be produced in an extremely simple manner by bursting a diaphragm between two vessels at different pressures. The pressure is pumped up in one compartment until it bursts the diaphragm and propagates a shock wave in the other. In this way temperatures of over 20,000° C. can be produced, and, what is equally important from the point of view of studying

the chemical changes which take place at such temperatures, can be produced quite uniformly through the tube in extremely short intervals of time. The shock wave travels through one millimetre in a millionth of a second, so by observing one narrow section of the tube, reactions of only a few microseconds' duration can be studied. Gases can be heated above 5000° C. in the time of ten molecular collisions. The method is very new and has so far been developed mainly in America. Already it has been used to study the kinetics of dissociation of nitrogen oxides and of the dissociation of iodine into atoms. It will undoubtedly be of great value in the study of the kinetics of high temperature reactions. It also has possibilities for chemical synthesis, and patents have already been taken out for the application of shock waves to atmospheric-nitrogen fixation, and the gasification of lignite dust, the synthesis of hydrocarbons from carbon monoxide and hydrogen, and of ammonia from nitrogen and hydrogen.

The second method of producing a high temperature rapidly in a gas was developed in this country and is known as flash photolysis. The energy is supplied to the gas by an intense flash of light lasting only a few millionths of a second. Usually the flash is operated electronically, and its intensity, during the short time in which it operates, is equivalent to about one million 100-watt electric light bulbs. The method has the restriction that the gas used must be capable of absorbing the light, but this can usually be arranged by the introduction of a suitable additive to the mixture. The theoretical temperature which can be attained in this way is several thousands of degrees Centigrade, but again, when we allow for dissociation processes, the real temperature is much less. Nevertheless, very high temperatures are readily achieved, and the method has the advantage that the energy is introduced in a very specific manner which makes the interpretation of the results easier. It has been particularly useful, when combined with spectroscopic methods, for the investigation of the short-lived species, such as free radicals, which play such an important part in high temperature chemical reactions. It is being used extensively in Sheffield and those interested may see the method in operation.

You may have noticed that in the last few sentences I have been rather reticent in mentioning exact figures for the temperatures which are attained, and the reason for this brings me to an interesting point which is of fundamental importance in very high temperature reactions. The fact is that the very meaning of temperature becomes less precise when we are dealing with a very rapidly changing system, as we always are when we study chemical change at high temperatures. When the temperature of a gas is raised, the individual molecules are caused to move more rapidly and all the other motions inside the molecule occur more rapidly, i.e. we make the molecule rotate faster, we make the atoms in the molecule vibrate more quickly, and we may even make the electrons in those atoms move faster as well.

When the gas is in equilibrium at a certain temperature the energy is distributed amongst these various motions, or degrees of freedom as they are called, in a quite definite way and we should get the same answer for the temperature however we measured it. If we put heat into the gas very suddenly however, by a chemical reaction or by one of the special methods which we have just been talking about, it may be a considerable time before this degree of equilibrium is reached—in fact, the greater part of the chemical change may be over before we have anything which can really be called a real respectable temperature. The energy passes into rotational motion in a few collisions, but it may take thousands of collisions with other molecules before the energy gets into the vibrations to any extent, and even longer—in some rather special cases seconds—before the electrons of the atoms pick up their fair share of energy. The rates of energy transfer between molecules and the various kinds of motion in these molecules are therefore of great importance in very fast reactions, and in discussions of such reactions it is quite common to hear talk of translational temperature, rotational temperature, vibrational temperature and

electronic temperature, all of which may be very difficult in the same system at the same time.

Even in such a familiar object as the bunsen-burner flame these questions are quite important. The flame consists of three parts, the inner cone, containing cold, unburned gas; the reaction zone, the narrow luminous zone surrounding the inner cone, where the reaction takes place; and the outer cone, containing the burned gases at temperatures approaching 2000° C. which are essentially at equilibrium. The reaction zone is therefore where all the chemistry takes place, and yet it is so thin that the gases are through it and out at the other side in about one ten-thousandth of a second. In this short time hydrocarbons and air are broken up, undergo an extremely complex series of reactions and are converted into water and carbon oxides. Each molecule, on the average, suffers 10^{10} collisions a second, so that, even in the short time it spends in the reaction zone, it will have the chance to collide one million times. Considering the involved transformations which it must undergo, however, one million collisions is not a lot, and certainly not enough to maintain a true temperature equilibrium throughout the process. For this reason we really know very little about the mechanism of this, one of the oldest chemical reactions known. Certainly there are some odd chemicals taking part—the light which you see from the reaction is nearly all emitted, for example, by the molecule C_2, which is unheard of in ordinary chemistry at lower temperatures.

If we measure the temperature of a molecule like this, whilst it is in the reaction zone, we get very peculiar results confirming the ideas which we have just been talking about. Although the calculated temperature on an equilibrium basis would be around 2000° C., the temperature measured considering only the rotations of one of these molecules may be as high as 10,000° C. The study of these elusive chemical substances and of the transfer of energy between them is one of the most challenging frontiers in the science of high temperature chemical reactions.

Are we now approaching the limit to what can be done in the attainment and use of high temperatures, or are there great advances yet to be made? Thinking along the lines which we have so far followed it would seem that the limit has nearly been reached. Experience of scientific development in the past, however, has taught us that prophets who have been presumptuous enough to answer questions of this kind have almost invariably been too pessimistic—whether they were sober scientists or imaginative philosophers. Some new twist is found in an old and well-established cul-de-sac and the path is once more opened wide. This has indeed already happened, and the term 'high temperature' has now acquired a new meaning on this planet. The temperature of the atomic bomb fireball when it is forty-five ft. in diameter is 200,000° C.; the hydrogen thermonuclear reaction attains a temperature of ten million degrees Centigrade; and other thermonuclear reactions, not yet attained on earth, such as those of helium, carbon and oxygen, cover the range 10^8 to greater than 10^{9}° C., temperatures comparable with the interiors of the hottest stars or anything else in the universe. Like the chemical reactions which we have been considering, the rates of nuclear reactions are temperature-dependent, and therefore, if we can get the temperature high enough to overcome the activation energy barrier, our nuclear reaction will proceed at a measurable rate and maintain the temperature for us, or, as more commonly happens at present, go explosively to much higher temperatures.

This possibility has very naturally caused an increased intensive interest in all methods of producing very high temperatures. Whether the reaction D + D = He should rightly be called a high temperature *chemical* reaction is a matter of one's point of view, but since the chemist's most early ambition was to discover the philosopher's stone, his interest in high temperatures must inevitably increase should a very high temperature turn out to be the philosopher's stone itself.

ARCTIC PLANT GROWTH[1]

by

DR. J. WARREN WILSON

ONE of the most striking features of arctic lands, to the visitor from temperate latitudes, is the poverty of the vegetation. Large areas are virtually devoid of plants, and elsewhere the vegetation is often open. Trees and (except in favourable sub-arctic localities) shrubs are absent; heathy and herbaceous perennials predominate. Typically, these are of species that are small in stature and leaf size, and in arctic regions the plants become dwarfed—in both vegetative and reproductive parts—compared with plants of the same species from further south (1, 2, 3). Failure to flower is not uncommon, and a large number of species normally fail to set seed (1, 2, 4). This general impoverishment of growth is an important characteristic of arctic vegetation, and it must be a basic task for arctic plant ecology to discover the reasons for its occurrence.

In this paper the view is put forward that the immediate cause of this poverty is an exceptionally small amount of annual growth. Evidence is presented on growth rates, and an attempt is made to show how the arctic environment, by affecting the physiology of the plants, brings about this depression of growth. The argument is based on observations made at Jan Mayen Island (71° N., 9° W.) and Cornwallis Island (75° N., 95° W.), and it is not known to what extent it may apply in other arctic regions. Nor is it suggested that low growth rates are the only cause of the poverty of arctic vegetation; for example, open vegetation may result from soli-fluxion effects, and dwarfing of plants from genetic adaptation of arctic ecotypes. It is certain, however, that the extreme depression of growth rates which is described below must powerfully contribute to impoverishment of the vegetation in arctic regions.

ANNUAL GROWTH

Evidence on rates of annual growth can be obtained from woody plants which form annual rings. By counting these rings, arctic explorers long ago discovered that the small, more or less prostrate plants of polar lands are often of considerable age (5, 6, 7, 8). There are records, for instance, of *Vaccinium uliginosum* plants over 80 years old, of *Salix arctica* over 150, and of *Juniperus communis* over 300 and exceptionally reaching 544 years of age. The annual rings are narrow by temperate standards: around 0·1 mm. thick, and generally composed of a very few layers of cells. Although these facts suggest that growth rates must be low, no information on dry weight changes was collected in these early investigations.

It is, however, a simple matter to determine the dry weights of the plants as well as their ages, and hence—if a sufficient number of plants is examined—to estimate the annual growth in terms of increase in weight. An analysis of this type, carried out on *Salix arctica* plants from two habitats at Cornwallis Island, suggested that growth was exponential for plants up to twenty years old, and that during this period the annual increment was about one-third of the total plant weight; increases of this order are attained in under a week by plants growing in favourable, temperate conditions (9). The data showed that at the end of twenty years' growth the weight of individual *Salix* plants amounted typically to only a few grammes.

In one of the habitats studied—a 'barren'—*Salix* was virtually the only plant

[1] Based on a paper delivered to Section K (Botany) on September 5, 1956, at the Sheffield Meeting of the British Association.

present, and it is possible to calculate the productivity of the area; that is, the weight of plant material produced annually. The value obtained is 30 kg./ha. Yields for temperate regions (10) are of the order of several thousand kg./ha.—roughly a hundredfold difference. The size of this difference suggests that, meagre as the arctic data are, they represent a real feature: an extreme depression of annual growth under arctic conditions.

One of the causes of this small annual increment of growth is, undoubtedly, the shortness of the growing season. At Cornwallis Island and in other high-arctic lands, activity is confined to a two-month period; and even in the most favourable parts of the Arctic, only four months are available. For the rest of the year the plants in many cases lose their leaves, generally become covered with snow, and inevitably are subject for much of the time to temperatures below freezing-point and nights that are long or continuous. No one has studied the behaviour of arctic plants in their winter environment, but it seems likely that there will be a loss of weight over this period; and it has been shown (11) that the carbohydrate content of overwintering leaves of *Vaccinium vitis-idaea* in Lapland becomes reduced to a very low value just before the snow clears in spring. Probably, overall loss in weight is relatively small—carbohydrate concentrations in stems and roots remain high even at the end of winter (11, 12)—but, even so, this unusually lengthy period of dormancy must tend to curtail annual growth.

Summer Growth Rates

Although arctic plants make relatively little growth over the whole year, they give the impression of being very active during the brief summer. Both Seward (13) and Wulff (quoted in (14)) refer to the 'explosive development' of arctic vegetation in spring, and Sorensen (15), describing the development of snow-patch vegetation in Greenland, states that its 'rapidity is unparalleled within the phanerogamic vegetation of the globe'. Rapid development does not, of course, necessarily involve fast growth in terms of dry-weight increase; but several authors (16, 17, 18) have stated that the growth of arctic plants in summer is strikingly rapid, due—it is suggested—to more or less continuous assimilation under 'midnight sun' conditions. However, quantitative studies do not bear out these suggestions of rapid growth.

Thus, observations at Jan Mayen and Cornwallis Islands on the increase in dry weight of detached leaves of *Oxyria digyna* placed on moist sand give net assimilation rates of 0·3 to 0·7 g./sq. dm./week. These contrast with values around 1·5 for detached leaves in temperate regions (19, 20).

Detached-leaf methods are open to criticism, since various physiological changes occur as a result of detachment; but analysis of the growth of whole plants has yielded similarly low values in the Arctic. For example, R.G.R.s (relative growth rates) of plants of *Oxyria digyna* and *Brassica rapa* grown from seed on fertilised vermiculite at Cornwallis Island (75° N.) were about one-third of those of plants of the same species on the same soil at Montreal (40° N.). These R.G.R.s were calculated on a daily basis; since they accumulate in compound interest manner, small differences become important over a period of many days. The three-fold difference in R.G.R. found in these observations would result in approximately a 15-fold difference in plant weight in the course of one month, and a 250-fold difference in two months.

It is unlikely that this depressing effect of the arctic climate is a result of the low light intensities, for the leaf areas of the arctic plants tended to be smaller, relative to total plant weight, than those of the plants grown at Montreal (suggesting more favourable carbohydrate relations), and analyses show that sugar levels in arctic plants generally are high. Probably, low temperature is the factor responsible.

In addition to this unfavourable climate, it seems that soils in arctic regions are

relatively infertile. In a pot experiment comparing the growth of *Oxyria* plants in a uniform arctic climate but on various soils collected at Cornwallis Island, the R.G.R. on fertilised vermiculite was greater than that on any of five native soils, and was almost ten times the R.G.R. on the most typical, 'barren' soil.

It seems, therefore, that even during summer the growth of arctic plants is depressed by both climatic and edaphic factors to rates which are extremely low compared with those of plants in temperate regions. This conclusion is based on pot experiments, but it is confirmed by samplings from field populations at Jan Mayen Island, which give, for various species, R.G.R.s around 0·01 to 0·03, contrasting with values of 0·06 to 0·17 for plants under favourable, summer conditions in temperate lands (9).

Soil Fertility and Nitrogen Relations

It is probable that differences in vegetational richness from place to place within arctic regions are most immediately determined by soil fertility rather than by climatic conditions; and it is worth enquiring what particular feature of the soil is effective. A pointer is given by the strikingly vigorous growth that is present in places manured by animals—around lemming and fox burrows, beneath birdcliffs, in polar bear lairs; here the vegetation becomes lush, and the plants are larger and brighter green than elsewhere. Even odd bones lying on barren ground may bring a local development of a few flowering plants and some moss. This stimulation of growth in places where there has been a special enrichment with nutrients suggests that the general infertility of arctic soils may be a result of nutrient deficiency.

Pot experiments at Jan Mayen and Cornwallis Islands confirm that *Brassica* and *Oxyria* plants grow far more rapidly on manured soils (from fox burrows and bird cliffs) than on even the richest of unmanured soils of these islands.

To find out which nutrients might be most seriously limiting, plants were grown on soils artificially fertilised with nitrogen, phosphorus, and potassium salts. Whereas nitrogen addition resulted in greatly increased growth, phosphorus and potassium had little effect except when nitrogen had also been added. It seems, therefore, that the infertility of the laval, agglomeratic, and limestone soils examined in these experiments may be chiefly due to nitrogen deficiency. The nitrogen contents of arctic soils, though exceptionally rising to a little over 1 per cent, are generally below 0·1 per cent; and of this nitrogen, only a relatively small proportion is in a form available to plants (12, 21, 22, 23).

Analyses of the plants grown in the fertilisation experiments mentioned above showed that their nitrogen concentrations were uniformly low—presumably representing the deficiency level—except in plants grown on soil fertilised with nitrogen, which had, as was to be expected, much higher nitrogen concentrations. Surprisingly, however, in plants grown on native soils rather constant nitrogen concentrations, at the deficiency level, prevailed throughout—even in the vigorous plants grown on birdcliff soils. This constancy of nitrogen content seems to be a general feature of plants growing at Jan Mayen. Numerous leaf samples collected from a wide range of habitats showed remarkably uniform concentrations of soluble and total nitrogen within each of four species; there was no trend of increasing leaf nitrogen with increasing soil nitrogen. The simplest explanation of this constancy seems to be that the growth of plants on even the richest soils, though faster than that of plants on poorer soils, is still limited by deficiency of nitrogen.

In the presence of such general nitrogen deficiency, it is not unexpected to find high carbohydrate levels. Further factors contributing to these are the continuous daylight, which permits photosynthesis throughout the twenty-four hour day (18, 24), and the low temperatures, which depress respiration more than photosynthesis (25). Sugar levels especially are high—often around 20 per cent (dry weight) in the leaves. Analyses of material from various habitats at Jan Mayen suggest that

there is, as with nitrogen, a uniformity of sugar level within species; in this case, however, the constant level is probably a 'saturation' level (26, 27). There is evidence that these high sugar concentrations depress the rate of photosynthesis.

Analysis of soils at Jan Mayen confirms that there is a general correspondence between soil-nitrogen concentration and vegetational luxuriance, and there is little doubt that nitrogen supply is the most important factor immediately controlling the distribution of the various vegetation types on this island. It is important to understand, therefore, how the soil-nitrogen level is determined.

Soil nutrients other than nitrogen are derived from the rocks from which the soils are differentiated, and seem to be present in adequate quantities in arctic soils (23, 28). Nitrogen, on the other hand, is accumulated either from precipitation—and the rain and snow of arctic regions contain relatively little combined nitrogen—or from fixation of atmospheric nitrogen by soil micro-organisms. Leguminous plants are rare in the Arctic, and nodule-forming bacteria seem to be generally lacking from the soil (29); mycorrhizas are formed by Ericaceous and other species, but no direct evidence of nitrogen fixation by them is available (30, 31); and it seems that free-living micro-organisms may be the most important source of combined nitrogen. *Azotobacter* is present in soils at Jan Mayen (12), but its activity is reduced by low temperatures; hence, presumably, the generally low nitrogen levels in these arctic soils.

Since these micro-organisms depend for their activity on organic materials provided by dead plants, nitrogen will be fixed most rapidly where the vegetation is richest. In such places, also, leaching will be least effective. It is to be expected, therefore, that nitrogen concentrations in arctic soils will be related, through micro-organism activity, to the vegetational luxuriance in any habitat. In confirmation, quantitative observations in five different habitats at Jan Mayen revealed a general correlation between vegetational productivity, nitrogen and organic matter contents of the soil, numbers of bacteria and fungi, and rates of soil respiration and ammonium utilisation. It seems that vegetational luxuriance and soil richness build up in balance with one another to a level which is in some habitats low, in others relatively high. The level which the ecosystem in any habitat does reach must be determined by some basic factor outside this system of soil-vegetation relationships.

Exposure and Plant Temperatures

At Jan Mayen, the distribution of the various vegetation types is closely related to the degree of shelter from wind. On windswept deserts and hilltops only the poorest growth is produced; on hillsides there is a richer, mossy vegetation which may be more or less closed; and in sheltered places—especially where these face south and are at low altitudes—there develops a continuous sward composed mainly of flowering plants. In excessively sheltered places deep snowdrifts may persist late into the season, and result in an impoverished growth, but, ignoring this effect and the local results of manuring, there is a clear relationship between vegetational richness and the degree of exposure or shelter. Soil analyses indicate that nitrogen concentrations build up to the highest levels beneath the rich vegetation of sheltered areas. Thus wind strength—as determined by microtopography—is of basic importance in controlling the distribution of the various types of ecosystem at Jan Mayen.

Laboratory experiments in temperate regions have shown that the growth of plants is reduced by wind, and the same effect occurs under arctic conditions. Turnip seedlings grew faster in a sheltered hollow at Jan Mayen than on the top of a hillock nearby (given the same soil), and detached leaves of *Oxyria* placed in the same two sites showed a 30 per cent difference in assimilation rate.

It is unlikely that this retarding effect of wind on the growth of plants at Jan Mayen is due to a lowering of the water status, for mist and rain are prevalent

throughout the summer, and the soil, although porous, is kept moist. It seems more likely that a cooling effect is involved; especially since the species characteristic of exposed areas are of northern distribution type, while those found in sheltered places are sub-arctic in character, and may accordingly be expected to require relatively warm conditions.

Both at Jan Mayen and in Swedish Lapland (32) it has been found that air temperatures during the daytime may be a few degrees warmer in sheltered hollows—where the air, moving slowly, can be warmed by the soil surface—than in exposed places. More important than this, perhaps, is the direct effect of wind on leaf temperatures.

Thermocouple measurements of the temperatures of *Salix arctica* leaves at Cornwallis Island (33) show that in clear weather the leaves are warmer than the air around them to an extent which depends on the angle of insolation and other factors, but which commonly amounts, during the summer, to several degrees Centigrade. This warming effect is present even at midnight during the 'midnight sun' period. Continuous records of the temperature of a leaf showed that crude sheltering from the wind—by means of a small sheet of cardboard placed on the windward side—resulted in a rise in leaf temperature of over 4° C. in two minutes. On removing the shelter, the leaf temperature fell to its original value. Again, leaves near the ground were warmer by several degrees than leaves at a small height above the ground, both because of the normal air temperature gradient, and also because the difference between leaf and air temperatures was greatest in the sheltered layer near the ground, where wind speed was reduced.

Differences of a few degrees Centigrade, or even of a fraction of a degree, may be of considerable importance to plants in an arctic environment; where temperatures are only a little above zero, small temperature differences have a great effect on physiological activity, and arctic vegetation reacts very sensitively to small changes in mean temperature (34). It may well be, therefore, that the basic control by wind of vegetational distribution at Jan Mayen is exerted through an effect on plant temperatures.

The views put forward in this paper can be summarised as follows. The poverty of arctic vegetation results primarily from an extremely low amount of annual growth, which in turn is due both to the shortness of the growing season and also to the slowness of plant growth even during the brief summer. These low growth rates result from the low temperatures—especially as aggravated by exposure of the plants to cooling winds—and, at least at Jan Mayen, from widespread deficiency of nitrogen, which arises through inhibition of soil microbiological activity by the low temperatures and the poor supply of organic material.

The lecture on which this paper is based was illustrated by diagrams giving much of the original data referred to. It is hoped that these results will be published in future volumes of the *Journal of Ecology*.

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VIGILANCE

AT THE Sheffield meeting of the British Association, Section J (Psychology) spent Friday, August 31, 1956, in discussing problems of vigilance under the chairmanship of Professor J. Drever (University of Edinburgh). Dr. N. H. Mackworth (Medical Research Council, Applied Psychology Research Unit, Cambridge) discussed the general factors which affected vigilance under experimental conditions. Mr. E. Elliott (Admiralty Research Laboratory) dealt with the measurement of the level of alertness by the method of effective threshold. Mr. D. E. Broadbent (M.R.C. Applied Psychology Research Unit, Cambridge) surveyed alertness in active as well as passive vigilance tasks, and Dr. D. H. Irvine (Army Operational Research Group) reported on investigations in industrial and clinical situations. Finally, Mr. D. Wallis (Admiralty) opened the discussion which was continued by Dr. D. C. Fraser (Institute of Aviation Medicine) and Dr. P. Bakan (M.R.C. Applied Psychology Research Unit, Cambridge). The following account of the symposium has been prepared by Dr. N. H. Mackworth.

SOME FACTORS AFFECTING VIGILANCE

By Dr. N. H. Mackworth

In the country called Science there is one of these New Towns called Psychology. Today is an exciting occasion because a number of us who have been visiting this far-away city of Psychology have come together to compare notes and to exchange travellers' tales. This is primarily so that you can hear what we think we have seen so far. This occasion is also an opportunity for us to get our ideas straight, to plan future journeys with your comments in mind.

When we visit this city of Psychology, all of us have gone to the most recently built and scientific section of the town which houses healthy human beings, a neighbourhood which is best described as that of Experimental Psychology. Nowadays even this is a large area and we therefore want to pinpoint the discussion rather more closely. I would narrow this down by saying that we are going to draw a picture for you of just one building in the neighbourhood called Experimental Psychology—one house with the name Vigilance painted on the door. The best way to find this building is to say that it stands at the intersection of two of the most interesting broad avenues in the city—the intersection between Motivation and Environment. Our existing knowledge of human alertness or vigilance is a large and rambling building which stands at the cross-roads formed by the intersection between the human factors implied by the term *motivation* and the physical factors arising from the *environment*, which is taken to include such factors as occur in the working equipment as well as those due to the general surroundings.

I shall try to give you a preliminary sketch plan of this building called Vigilance. This will not be an architect's drawing, all squared away with every room exactly specified. If we regard the factors affecting vigilance as separate rooms in the vigilance building, I can perhaps list some of the rooms with some indication of their relative importance. We do know a certain amount of this building, but each of us has been looking at different rooms—in the sense that each of us here today has studied different factors. As the morning goes on, further rooms may be added or even taken away, but I have the feeling that there are some rooms in the vigilance building that have not yet been seen by anyone.

No verbal definition can therefore be exact, but it is perhaps useful to suggest that the term *vigilance* could be regarded as *a state of readiness to detect and respond*

to certain specified small changes occurring at random time intervals in the environment. The subject has been instructed to detect these signals and in that sense he is therefore expecting them, but on the other hand he does not know *when* they will occur. He knows what to look for, but not *when* it will happen. In practice, the extent of this alertness to detect and respond is usually measured by expressing it in one of two ways. The first method is to find the proportion of such signals that passes unreported, which gives the probability of neglecting a given signal. The second method is to find the size to which the given signal has to be increased before it is detected and reported.

The situation then usually involves a searching for certain definite and specified signals which are difficult to detect and are occurring at unpredictable time intervals. In terms of *real life* the question could be asked: 'Why do people fail to notice and act on faint lights or sounds, or small deviations in the position of a pointer—signals they are normally able to detect?'

In the *laboratory*, a vigilance task is a situation which tries to do two things. First of all it provides a form of work in which people are likely to be less watchful than they normally are, and secondly it measures the resulting changes that occur in the behaviour of these people. This gives us a chance of discovering some of the main causes in the work itself which lead to a loss of vigilance. What are, in fact, the basic environmental reasons for lapses of attention when these wanted signals are only just detectable?

I. Factors in The Working Situation

These causes of loss of vigilance are often in the working situation and the main environmental factors would appear to be associated with various aspects of *time* as regards the work.

1. *Frequency of signal.* Very often in real life the wanted signal occurs only occasionally. In some cases, days, weeks or even months may elapse before one of these events takes place. But even quite small alterations in the number of wanted signals presented in a given time can make a great deal of difference to the probability that any such signal will be noticed. For example, in a recent paper by Deese (1955) increasing the number of wanted signals per hour from ten to forty doubled the chance of detection and raised the percentage of detected signals from the very low figure of about 45 per cent to about 90 per cent. Woodhead and Bowen (1956) have confirmed this effect of simply increasing the frequency of the wanted signal. They found that people were more likely to notice signals when there were ten signals per hour than when there was only one such signal per hour.

2. *Inter-signal interval.* The time that elapses between one wanted signal and the next appears to be of very great importance. In fact, recent unpublished work by Baker (1956) would suggest that *this spacing of the wanted signals along the time scale is probably the most important factor of all in determining whether or not a decline in the probability of detection will occur as time goes on.* Baker tested eighty people with a situation in which small wanted signals occurred at fairly *regular* time intervals, and compared the performance when similar signals occurred at quite *unpredictable* time intervals. The *regular* signals showed no falling off in the probability of detection during one hour. With a further thirty people the *irregular* series showed, however, a very marked decrement after half an hour had elapsed. This indicates that temporal irregularity may be an essential feature to produce decrement during a monitoring spell. The results were that the *regular* signals gave an 80 per cent probability of detection during the whole of a one-hour spell of work, whereas the *irregular* signals showed an 80 per cent probability during the first half-hour, but this figure fell to about 60 per cent during the second and third half-hours.

3. *Length of the working spell.* Many years ago Wyatt (1932) showed that the

incidence of rejects in an inspection task fell off after the inspectors had been about half an hour on the job. I found the same half-hour effect in a number of laboratory studies (Mackworth, 1950). Since then many other investigators have confirmed that a decrement may take place during prolonged vigilance—e.g. Broadbent (1953), Baker (1956), Whittenburg *et al.* (1956) and Adams (1956). The exact point in time at which alertness begins to deteriorate depends, however, on a whole number of physical and psychological factors; according to these circumstances the onset may lie anywhere between a few minutes to one hour from the start of the spell. I agree, however, with Elliott that the emphasis in research should no longer be on this decline with prolonged work, but rather on why the operator does not do what he could do. It would be useful to know the shortest rest that is effective in restoring vigilance.

4. *Duration of the wanted signal.* Broadbent (1953), Fraser (1953) and Adams (1956) have drawn attention to the importance for prolonged performance of the duration of the signal itself. A momentary reduction in alertness can only be detected if the signal is correspondingly brief. But a short-lasting signal is *not* an essential characteristic of reduced vigilance because Saldanha (1956) has shown in studies of accuracy during prolonged performance that a marked decline in alertness can occur even when the given signal remains until the subject has made his response. Similarly, Bakan (1955) obtained a marked decrement even though each signal was spread over 5 to 45 seconds.

Before we consider any other factors, how do these *time* factors apply to real industrial inspection jobs? There is a great need now to consider in general how far these results apply to industrial, as distinct from military situations. For instance, it is often very difficult to make inspection work entirely automatic, even when production is largely a matter of machines rather than men. A good deal of inspection work entailed in the vast output resulting from automation is still going to be done by the human eye, despite the great advances made by the electronic industry. In such inspection tasks spells of work are usually two hours or more in length, while rejects occur at unpredictable time intervals.

How *fast* do events occur in industrial inspection tasks? The fastest I have seen is work in which people look at over a quarter of a million objects in one working day. A good operator inspecting lighter flints can deal with about three hundred thousand flints in a day—i.e. about eight flints a second. At the other end of the scale, for obvious reasons one of the slowest forms of work requiring great care is the inspection of intravenous ampoules in the pharmaceutical industry for glass spicules, fibres and specks of carbon. Here the skilled operator may consider no more than three thousand ampoules in one day. This is then a range of a hundred to one; with, for instance, 10 per cent rejects, the fast rate of working represents 3,750 wanted signals per hour, a much faster rate than many military tasks.

This brings up the point that when we are continuing our search for causes of failure in the work itself we have to consider not only the few wanted signals, but also the many *unwanted* signals. Remarkably little is known about the possible factors affecting alertness that might arise from these unwanted signals. It may, for example, be that a regular repetition of unwanted signals that the man is trying to neglect is just as harmful for alertness as is this *irregularity* in time for the *wanted* signals. Secondly, the unwanted events are usually so much more frequent, yet nothing is known about the effect of systematically increasing the proportion of these without altering the number of wanted events per hour. To give an example, the wanted signals on the wartime radar screen were the echoes from a submarine, but in the Bay of Biscay, out of the suspicious signals investigated, thirty were from fishing vessels for every one from a submarine. This brings out the third point about the unwanted signals, because the echoes from the fishing boats looked very like those from the submarine. False alarms are often very like the real thing. This

similarity between real and false signals is believed to be a factor in the decline in vigilance. But no one has systematically varied this similarity to see whether constantly having to neglect false alarms is more harmful to vigilance when these are very like the real thing than when they are not. Research is therefore needed on these three aspects of the *unwanted* signal—regularity, frequency and similarity to the wanted signal.

II. General Environmental Effects

Several points are clear about the effects of the general surroundings on alertness. Broadbent (1953) has proved that even moderately *noisy* circumstances can reduce watchfulness, just as Pepler (1956) has confirmed that surroundings that are even moderately *hot* do the same.

It is most interesting, too, that *isolation* can have the same effect. Fraser (1953) showed that isolated subjects are less alert than those working with someone nearby. Cutting down the so-called distractions reduced the ability to detect faint signals. In general, it seems that lack of sensory signals may lead to a decline in this readiness to detect and respond to small changes in the environment. Deese (1955) and Bexton *et al.* (1951) have emphasised the necessity of having at least the basic minimum of sensory stimulation if people are going to maintain their vigilance. But we have Dr. Bakan with us today, who is one of the leading workers on vigilance problems in the United States. He has recently come over to Cambridge to continue this work with the help of the National Science Foundation. As he will be working on the problem of whether a decreased sensory input to the eyes will reduce vigilance in a hearing task, I am hoping that Dr. Bakan will later this morning be telling us something of this exciting work. Such investigations are likely to add to our knowledge of the nature of the differences between sleep and wakefulness, so that now is a good time to mention some recent work by Wilkinson (1956) on the effects of lack of sleep on vigilance.

III. Motivational Effects

(*a*) *Reduced Motivation.* Now we are in the difficult region of the factors related to human motivation that affect vigilance. We would all agree, however, that loss of sleep produces a state of reduced motivation in the sense that behaviour is then less directive and purposeful. Wilkinson has demonstrated a loss of alertness when sleep-deprived people are asked to report faint short-lasting signals. After normal sleep, the subjects saw most of the signals, and missed no more of these at the end of a run of 45 minutes than at the beginning. However, *after one night without sleep* the same men started off apparently fully alert, but during the 45 minutes of the test they showed a steady and marked increase in the number of the signals not detected and reported—despite the fact that at the instant of the signal the men were known to have been looking in the right direction. This decline in detection ability was not due to the men being actually asleep; only about 10 per cent of the missed signals were neglected for this reason.

(*b*) *Increased Motivation.* In some of my own experiments (Mackworth, 1950) I have shown that there are at least two ways in which alertness can be sustained by motivation rather than environmental changes. The first method is to give the subject a small dose of amphetamine sulphate, or two benzedrine tablets, one hour before starting the task, and this keeps vigilance at the normal level for at least two hours. Secondly, motivation can be increased by the more usual procedure of providing immediate knowledge of results during the task itself. It is possible to keep alertness at the initial level for at least two hours by giving people some basis for judging their own performance as they proceed. The subjects are told the moment they miss a signal, and the detection of a signal is confirmed.

Finally, the theoretical side needs some comment. Originally I thought that the

primary factor in many situations leading to a loss of alertness was the absence of such progress information reinforcing the correct responses (Mackworth, 1950). I thought at that time that a further and less important factor was the irregularity of the spacing of the signals in time, leading to moment-to-moment changes in the level of expectancy due to this unpredictable interval between signals. Deese (1955) holds that this factor of expectancy is the only factor. This recent work by Baker (in which irregular spacing of signals could by itself produce a decrement in vigilance) makes me agree with Deese that the element of expectancy is of the highest importance. But I still think that the absence of reinforcement by knowledge of results is a factor of some importance which cannot be entirely disregarded.

I have not said anything about individual differences. But perhaps our Chairman can be persuaded to describe how he is starting his crucial studies on the relation between performance during vigilance tasks and the electroencephalograph readings taken from the same people at rest and during the vigilance task itself.

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AUDITORY VIGILANCE TASKS

By E. Elliott

It is our common experience that we are not fully aware of all the sounds which reach our ears. For instance, we are not generally aware of the tick of a clock, though there may be no difficulty in hearing it if we set out to do so. If, however, we could control the physical dimensions of such a sound, we should expect to be able to capture a man's attention at any time by making the sound stimulus big enough. In other words, we can conceive that there is always a sort of threshold for any particular stimulus. I shall call such a threshold an 'effective threshold', where some maximum sensitivity is measured.

I propose discussing the problems of measuring these effective thresholds, and I will show how their measurement contributes to our understanding of vigilance tasks. Although the discussion will be restricted to certain auditory tasks, it should be possible to apply the same arguments generally to visual and other sensory vigilance tasks.

First let me make the concept of an effective threshold clearer by referring to Fig. 1.

Here we have a largely imaginary graph plotting a man's effective thresholds for

some particular stimulus against clock time. To read the graph you take any point on the curve and say that, at the indicated time (e.g. 12 o'clock) a stimulus as great as, or greater than that indicated (say x units) would have produced a response; a stimulus weaker than x units would not have produced a response. The graph indicates that you would have to use a very big stimulus to get a response during deep sleep, perhaps a smaller stimulus during lighter sleep; during waking hours effective thresholds might maintain an approximately constant level somewhere above a normal threshold, which is the base-line of the graph. Of course, we ought to suppose that there will be large fluctuations about the imaginary general levels I have shown.

No-one knows how to make measurements continuously which would lead to an accurate construction of such a graph, though there must be psychological or physiological processes corresponding to such hypothetical effective threshold curves. However, we can make some sort of estimates of discrete points along such curves.

The first thing we have to do is measure a normal threshold, which gives a base-line against which effective thresholds are to be measured. To make an effective threshold measurement we borrow the ascending method-of-limits from normal

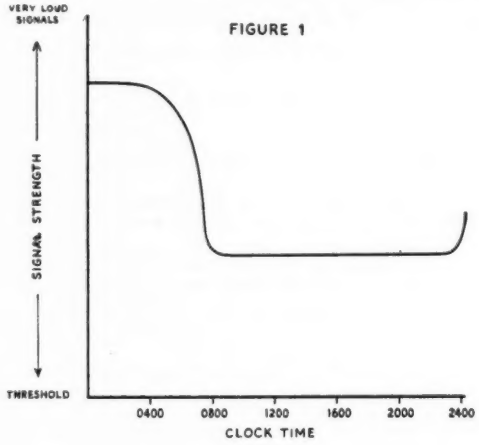


FIGURE 1

psycho-physics and use it with a large time interval between stimulus steps. For example, suppose our stimulus is a tone pulse presented against a noise background. We present a stimulus at a threshold signal-to-noise level, which we will arbitrarily call 0 decibels. If we get a response we record an effective threshold of 0 decibels; if not, we wait, say 30 seconds, and present a stimulus 1 decibel above threshold. Now if we get a response we record an effective threshold of 1 decibel; if not, we wait again and present a stimulus 2 decibels above threshold. We go on like this until we do get a response.

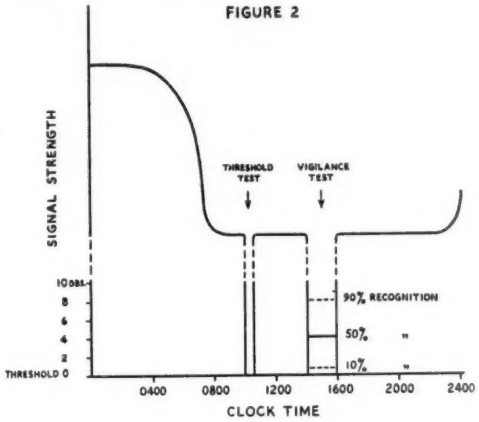
The subject of this sort of test should believe that he is likely to hear only single stimuli; and if the interval between stimulus steps is fairly large (30 seconds) this will be the impression he gets, though in fact there may have been a dozen or more stimuli in the ascending series. This, I think, is most important. If you try and use a straightforward method of limits, with the normal short interval between steps, and if you are proposing to use such a test repeatedly over hours or days, the subject sooner or later will get an idea of what is going on. Then he may delay his response until he has heard several stimuli; and estimates of effective thresholds will be too large and rather unreliable.

Fig. 2 gives a preliminary idea of how this sort of test works out.

This graph repeats the imaginary effective threshold curve of Fig. 1. But now

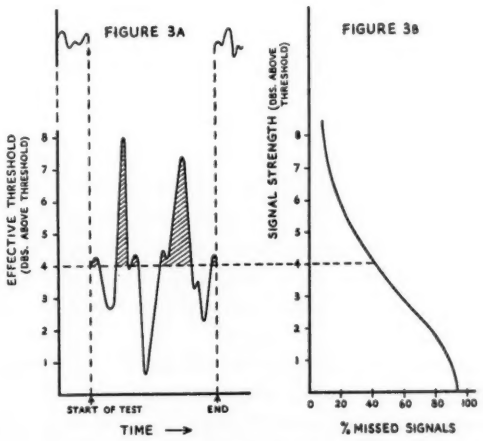
we suppose that, at 10 o'clock, our subject is given a normal threshold test, in which, by definition, his effective threshold is zero. In the afternoon he is given a test lasting two hours. Instructions for this test are, roughly, that the subject has to press a key as soon as he hears a signal; he is told that there will be only a few single signals during the two hours. In fact, each signal will be the terminal step in the ascending series just described.

FIGURE 2



There seem to be no trends in effective threshold level during such a test. If our subject was the mythical average man, his average effective threshold would be about 4 decibels above threshold. In only 10 per cent of the time would his effective threshold come within 1 decibel of threshold; and 10 per cent of the time it would rise above 8 decibels.

The relationship of practical estimates of effective threshold to the hypothetical curves of Figs. 1 and 2 can be shown more clearly perhaps by taking account of fluctuations in the hypothetical curves.



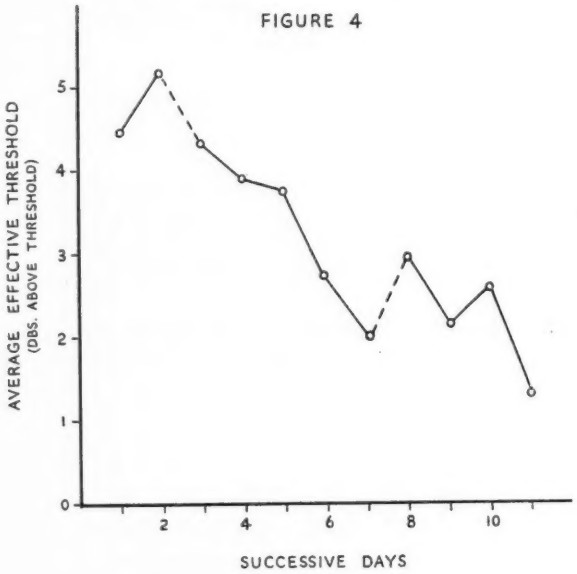
In Figure 3A I have drawn a hypothetical curve representing fluctuations during a 2-hour test. From this curve we can obtain the one in Figure 3B by simple calculations. We take any stimulus level (say 4 decibels); and now we calculate what proportion of the two hours is represented by the shaded areas above this level. We can do this for several levels. Now if we plot stimulus level against these calculated proportions of time when effective thresholds are above these levels, we get something like Figure 3B.

Now there are no available techniques for producing a graph like Figure 3A. But the technique I have just described does yield a curve like Figure 3B. Thus, if we make certain assumptions, we can argue that empirical curves like that of Figure 3B tell something about the unmeasurable curve of Figure 3A.

At this point I can show one great practical value of measuring effective thresholds. In the past, when vigilance tasks have been studied in the laboratory, the usual procedure has been to present subjects with occasional signals at fixed intensities. At the end of such tests the experimenter can calculate how often his signals were missed. This calculation gives just one point on a curve like Fig. 3B (e.g. if only 4 decibel signals were used, about 50 per cent would be missed). It is clear that measuring effective thresholds is much more satisfactory, since we get very much more extensive information.

As a further illustration of the use of the effective threshold technique I can quote some more experiments on detecting tone pulses in noise. I wanted to compare detection of signals presented at a slow rate with detection at a fast signal rate.

FIGURE 4



Accordingly, I measured effective thresholds about ten times in a two-hour test session, which a group of six subjects underwent every morning for about three weeks. In the afternoons of the same days the same subjects were given something like a continuous threshold test. This involved presenting signals about 1 decibel above threshold at a rate of 10 signals per minute; and this task went on for 105 minutes with a 15-minute break half way.

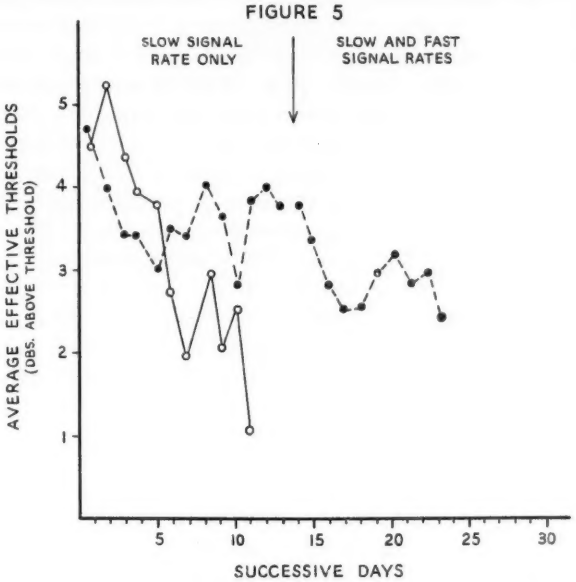
In Fig. 4 the daily average effective threshold for the morning sessions are plotted. The results of the afternoon sessions are not shown.

There is a large improvement as time goes on, and this improvement occurred to a greater or lesser extent in each subject's performance. This result was quite unexpected, but presumably it must be due either to large practice effects in the morning sessions or to a transfer of learning from the afternoon sessions.

To try and decide between these possibilities I repeated the morning sessions alone, using a new group of 12 men. After three weeks I kept the morning sessions going and added afternoon sessions with the fast signal rate, though, for administrative reasons, I had to cut the length of these to half. I went on with this new routine for a further three weeks.

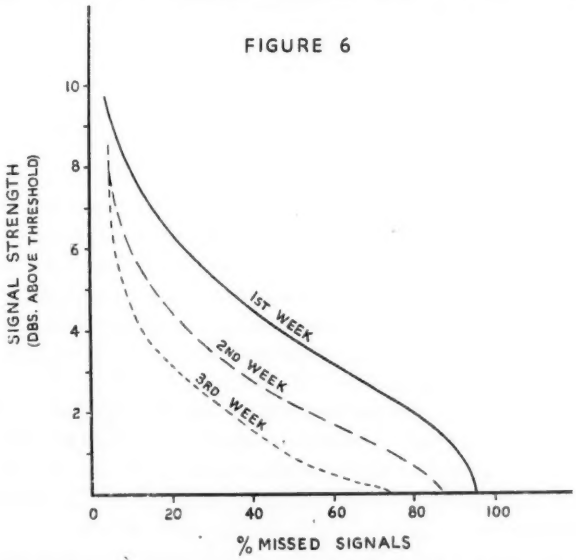
The data from the earliest experiments are repeated in Fig. 5 (solid lines). The new data (dotted lines) show a small increment during the first day or so of the experiment: but then the average effective threshold level stayed about constant

FIGURE 5



until the introduction of the afternoon test sessions. Then we see some further improvements. These are not so spectacular as they were in the earlier experiments; but this is not surprising since the dosage of fast rate tests was halved. Thus, although practice in the morning sessions itself seems to produce a little

FIGURE 6



improvement, I think the major effect here is a transfer from the afternoon to the morning session.

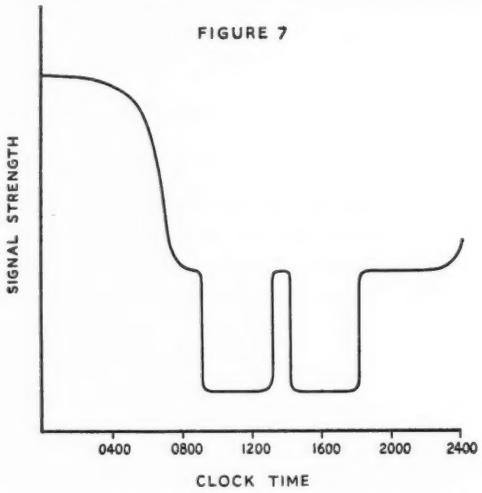
This conclusion is strengthened by examining further aspects of the data. First let us go back to the earlier experiments.

In Fig. 6 I have worked out the probabilities of missing signals at different intensities, and I have done this for each week's data. Again we see the improvement

in performance as the experiment progresses: but the three curves tend to merge at the higher signal intensities. The improvements are improvements in detecting the weaker signals, whereas throughout the three weeks, effective thresholds were always 8 decibels worse for 5–10 per cent of the time. The same finding held over the six weeks of the later experiments. Further, we noticed that the commissive error rate was roughly doubled immediately after the introduction of the afternoon sessions in the fourth week. With this additional information I think I can now explain what happened in these experiments.

With the fast signal rate the subjects knew that signals were imminent, and they were able to keep up something like normal threshold discrimination for the whole of the test. But there was nothing in the test to penalise or correct commissive errors, and subjects developed the easy habit of making them. Now people do not commit errors wilfully; they do so because they think they hear some signal, and, indeed, in the noise masking conditions which I use, there will be momentary fluctuations of the noise itself which will sound like signals. In short, in these tests subjects are more or less forced to listen down into the noise itself. If this habit transfers to the morning sessions, where there are few true signals, we expect more

FIGURE 7



commissive errors, but we also expect the true weak signal to be heard more readily. At the same time, since true signals are not expected very often, men will tend to relax from time to time, just as they did before changing their commissive error habits, and their effective thresholds will rise to quite high levels.

If this explanation is correct two conclusions can be drawn. First, in noise limited tasks, if you want to get optimum detection, you ought to encourage a high commissive error rate. Secondly, the really serious problems of vigilance relate to the larger peaks in effective threshold curves, which together may last perhaps 5–10 per cent of the total spell of duty. In many real, as opposed to laboratory vigilance tasks this apparently small incidence of high thresholds must lead to expensive mistakes and to dangerous accidents. And it is not at all easy to see what can be done about them.

Perhaps we might make some progress if we followed up the suggestions implicit in my opening statements. I was concerned with everyday behaviour, which involves attending at any moment to just one or two of innumerable stimuli, and, generally, scanning rather rapidly from one set of stimuli to another. When we ask a man to do a vigilance task, in effect we expect him to make radical changes in his normal behaviour; he must stop scanning, or, at least, he must learn to control his scanning so that the task signals are never wholly rejected.

As far as I know no-one who has been concerned with the problems of vigilance, has set about measuring the natural processes of attention which are distorted in vigilance tasks: all our measurements refer to the distorted behaviour.

Fig. 7 presents again the effective threshold curve shown in Figs. 1 and 2. But now we imagine that, for eight hours a day, a man is involved in an inspection job. During this period it is desirable that his effective thresholds approach threshold, though his natural living level may be very high indeed. All our laboratory measurements are concerned with these periods, which occupy so much of this picture, where behaviour is biologically normal. I think we should get a great deal of useful information if we started to study normal behaviour. And, since it is just about as easy to apply the effective threshold technique outside formal test periods as it is in the laboratory, there is no reason why we should not go ahead and investigate this untouched and, I think, highly relevant field of behaviour.

THE VIGILANT MAN AND THE ACTIVE MAN

By D. E. Broadbent

As this is by no means the first paper in the symposium, many of you will be in a mood to consider favourably the chief conclusion of vigilance experiments. I mean, that one can get tired of sitting still and receiving information. It is not necessary to demand great physical activity if one wishes to observe a drop in human efficiency. This serves as a great comfort to those who, like myself, have to be watched closely during working hours if any movement is to be detected.

They also show fatigue who only sit and watch.

However, it may not be quite fair to make a distinction between the active man whose hands and feet are busy moving controls, lifting objects, or assembling components: and the vigilant man whose role is merely to observe and detect. In these days one does not employ human beings because of their muscles. A worker, such as a dustman, earns his pay by his sense-organs and his intelligence rather than by being a mobile power-plant. A dustman has to detect dustbins of all kinds of shapes and sizes, which may be placed in a wide variety of different positions. No two neighbouring dustbins are likely to need exactly the same movements. It is this observation part of the dustman's job which makes it so difficult to replace him by a machine. Of course, he indulges in a lot of physical activity; but it would be a mistake to look simply at this activity and ignore the perceptual part of the work. In other industrial jobs, this may be even more true.

Now, in active work of this type there are often signs of reduced efficiency following long work periods. To take a recent example, in the London docks accidents are more frequent at the end of the morning than at the beginning, though the reason is not definitely known. Is it possible that this deterioration in the work is not due to fatigue of the muscles causing failure to achieve the desired movements? Rather, it may be due to failures of perception, failure to notice the unusual danger-signs when something unexpected happens. If this is so, the results we get from vigilance experiments are of interest not only for the vigilant man at his radar set or his inspection of finished products; but also for the active man driving a car, swinging a pick, or assembling some manufactured article. And in recent years quite a lot of evidence has accumulated which does suggest that failures of perception may occur even with prolonged active work. It is this evidence which I am going to talk about. First of all, let us take two experiments which showed rapid deterioration in active work. It is not easy to show such deterioration—*many industrial tasks are done as well after several hours as they are initially*. But these two jobs involve continuous observation, and they both reveal quite a quick drop in efficiency. The first, studied by Dr. Saldanha (1956) of Cambridge is the making of a series of settings using a Vernier gauge controlled by a small hand-wheel. The movements

are made just as quickly when the man has been working for some time; but the settings become less accurate after fifteen minutes. The man is looking at the scale and accepting as correct readings which in fact are not correct. This finding, apart from spreading alarm and despondency amongst our workshop staff, goes to show that one can get very rapid deterioration in active work which needs close observation.

Another similar study is by Siddall and Anderson (1955) from Farnborough. Tracking tasks, that is, the following of a target with a gun or pointer of some sort, do not always show much effect from prolonged work. But Siddall and Anderson constructed a tracking task in which the visual stimuli were made very monotonous. All the man had to do was to keep a pointer on a stationary mark by turning a handle at constant speed. If his speed altered, he would detect this by seeing the pointer move and could correct it. But such errors became more serious as the period of work went on. This may have been due to the man's muscles getting tired, but it seems likely that it was due to 'not noticing' the error: so, point one, *tasks with a strong perceptual element are particularly likely to show bad effects from prolonged work*, although by itself the evidence does not prove that it causes the decrement. Now let us consider point two: *the low efficiency one sometimes finds in vigilance tasks is not improved by making the work more active, while keeping the surroundings as monotonous as ever*. For instance, Whittenburg, Ross and Andrews (1956) in the United States compared two ways of administering Dr. Mackworth's Clock Test. In one case, the man had to watch a pointer ticking round a dial in a series of small jumps, and press a key very occasionally if the pointer gave a double jump. In the other case, the man had to press one key for double jumps and another key for the (very numerous) small jumps. This transformed the job into a very active one, but it made no difference to the accuracy with which the double jumps were detected. People still became inefficient after only half an hour's work. Wilkinson at Cambridge has carried out the other possible experiment of asking a man to press a key regularly during long periods, but to stop work for an instant when a signal appears. Under these conditions the man may work even when he should not. (This might correspond, say, to putting a hand into a machine even though it is in a dangerous position.) In short, *it does not seem to make any difference how much activity you demand from the man: if you are expecting him to notice occasional small differences in his surroundings, his efficiency may be low.*

The third point in my argument is that, in some cases, it has been proved *that muscle fatigue is not responsible for low performance found on active tasks*. For example, I apologise to those who have heard me on previous occasions describe the work of Adams (1955) in America: but I do not think one can summarise it better than by saying that he proved that *one can get tired watching other people work*. He used a task in which one followed a spot on a gramophone turn-table: and he compared the effect of rest-pauses right away from the task, and rests in which one watched somebody else do the job. It was better not to watch others. Another experimenter using the same task showed that changing hands did not eliminate the effects of prolonged work, so they cannot all be due to fatigue of the muscles.

These three groups of experiments, then, together with certain other evidence, make it likely that the findings of vigilance experiments apply to active tasks. Failure to notice is an important element in what is loosely called 'fatigue'. Or, if you prefer, prolonged work does not simply cause lack of co-ordination in movements: it may also cause an interruption in the flow of messages from the sense-organs before they have travelled very far through the brain. Now, this is of some practical importance. Consider, for instance, the five railway engine drivers who have so far been censured this year as being responsible for accidents. Are we really sure that failure to see a signal at danger can be prevented by exhortations to all, and punishment for those who fail?

But in addition the possibility of a parallel between vigilance tasks and active

tasks makes it easier to find out what is happening in vigilance. In experiments on the detection of radar signals or other occasional stimuli, psychologists are almost as badly off as nuclear physicists. They have a man sitting in front of their apparatus, but they only find out about his state at the instant when they hurl a signal at him: just as the physicist only finds out about the nucleus by bombarding it. In a pure vigilance situation there is no way of telling how the man is behaving during the time between signals. In more active tasks we can record what he is doing more continuously, and I personally *believe that this sheds a great deal of light on results from vigilance tasks*. For example, suppose one studies the kind of task in which a series of different stimuli are presented to a man, each stimulus being delivered after the previous response is made. In such a task the rate of work depends quite considerably on the probability of a particular stimulus occurring. If one type of signal appears very rarely (whether because there are a lot of different kinds of signals or because the different kinds are not presented equally often) the response to that signal is slow. If the man is familiar with the work, this slow response to improbable stimuli appears even at the beginning of the work period. In fact the *average* rate of work remains very much the same for quite long periods. It may improve in some people and under some circumstances, or it may possibly deteriorate under other conditions: but it does not universally or necessarily deteriorate. What does seem to happen is this. A man who is fresh works at a fairly even rate even when he starts each session. But at the end of the period he works unevenly: he has spells of quite normal performance, but occasionally there is a break in the flow of responses and he either does nothing for a second or two or else makes a mistake. In fact he works at the same average rate but in a series of spurts and pauses.

Some years ago it was pointed out that this feature of active work meant that a job in which the man set his own pace could be carried on for longer periods than work in which he tried to keep to the same pace as a machine: and this was proved to be so, first in the laboratory (Broadbent, 1953) and later by Conrad and Hille (1954) in an industrial job. But there is another implication. Suppose we think that the measurement of active work reveals what a man really does in a vigilance situation, during those times when we aren't giving him any signals and therefore do not know what he is up to. Suppose we put up the theory that he keeps looking at the radar screen, or whatever he is watching, at an average rate determined by the probability of seeing a signal there. But when he has been going some time, he will occasionally fail to look for a second or two; and then carry on again just as he did at the beginning of the session. Now these short gaps during which he is not looking will only matter if the signal which comes up is also very short. If it is, if it can appear and disappear during a brief spell of inattention without ever giving the man another chance of detecting it, then he is going to miss more signals after prolonged work than he did when he was fresh. But if the signal is a long one, or comes up repeatedly until it is seen, then his momentary aberrations will not matter and he has as good or bad a chance of seeing the signal when he is tired as when he is fresh.

So this means that there will be two distinct things to consider in vigilance. First, there is the average efficiency even at the beginning of the work period. This will be low if the signal is improbable, if the man is used to getting such signals only very rarely. And this we must do something about, possibly by putting in extra signals. But it has nothing to do with the drop in efficiency which may take place as the work period increases. The vigilant man will only get worse as time goes on if the signal he is looking for is of certain kinds: in particular, if it is very short-lasting and so can be completely missed without a chance of later detection. And this distinction, drawn from observations of active tasks, does seem to apply to vigilance experiments. There are cases, like those which Mr. Elliott has mentioned,

in which efficiency is low right from the start but gets no lower as time goes by. There are other cases, like the original experiments of Dr. Mackworth which started the interest in this subject, in which performance gets worse quite rapidly as the work period gets longer. I suggest that these two types of experiment are measuring different aspects of the man's behaviour: one his average level, and the other the lowest points his efficiency reaches. These 'lowest points' depend on the time for which he has been working, while the average does not.

Lastly, let me mention a very interesting task devised by Dr. Holland (1956) of the Naval Research Laboratory in the United States. He asked a man to watch a dial for a signal. But the dial is normally invisible: it can only be lit up by a flashlight by pressing a button. So it is possible to record from this button every time the man looks at the dial. If you do this you find that his rate of button pressing depends on the frequency with which he sees signals: he does not look so often if he hardly ever sees anything. And of course if the signals usually come about 2 minutes apart he looks more at that time and less just after a signal. This kind of combination of an active task and vigilance offers a good chance of checking on the view I have been putting forward. But there are two other reasons for mentioning this experiment at the end of my innings. First, the behaviour of man in this active-vigilance task is closely similar to that of animals in the type of learning experiment known as the Skinner box: which is responsible for the majority of modern work on animal learning. It is of very great theoretical interest that this should be so. And secondly, if active vigilance of this sort is possible it is more difficult to avoid the conclusion that the results of, say, Mr. Elliott's work are important even for those whose jobs seem far more active than that given to his subjects. A short time ago I saw a road safety propaganda film on T.V., in which it was said that accidents are caused by the thoughtless and selfish behaviour of thoughtless and selfish people: the problem was regarded as a purely moral one; a subject for propaganda or pep-talks. This approach does not seem conspicuously successful. I would prefer to supplement it by experimental investigations. So, for both practical and for theoretical reasons, I would make the admittedly large claim that vigilance is the most important single topic now being studied by psychologists.

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VISUAL INSPECTION AS A VIGILANCE TASK

By Dr. D. H. Irvine

In a recent address to the Institution of Production Engineers, Dr. Mackworth (1956) has pointed out that increasing emphasis will be laid on vigilance tasks and inspection tasks. Less and less will machines be fed and cleared by human operators, while the transport of material will become more automatic. Of course, some tasks will still be done by hand, even though it would be quite possible to design a machine to do them, simply because it would be uneconomic to do otherwise.

This situation occurs today in factories which now and again produce small quantities of specialist lines; biscuits for lifeboats and certain drugs, for example, are hand packed because they form a small part of the total output of the firms concerned. For the same reason some relatively simple machines requiring hand feeding and clearing will be retained. Nevertheless, the trend is towards machine minding, and there are still likely to remain many situations in which inspection of the finished product by human beings is necessary.

There are in existence today many situations in which machine minding or watching occurs, and these situations vary greatly in complexity. The simplest situation is one in which one operative is in charge of one machine, for example a machine wrapping toffee, or labelling tins. Sometimes the operative has to feed a load of material into one or more hoppers, but the major portion of his or her time is taken up by seeing that the machine is running correctly and clearing it or adjusting it when it goes wrong. In passing, it is worth pointing out that although it is often thought that machine-made products are very uniform and depend little on the operator, and also that output is dependent entirely on the speed at which the machine produces the goods, this is not so. It has been shown by Coakley (1950), for example, that sizes of ladies' hosiery varied greatly from operator to operator even when all operators were supposed to be producing the same sizes. Again, output depends greatly on the skill of the operator in setting his or her machine, and in noticing that something is wrong as soon as, or even before, the machine has actually stopped.

These comments apply with even greater force to more complicated situations in which the operator is minding several machines, or machines with a complicated display of dials, lights, warning sounds, etc. In textile weaving and many other jobs, it is common for the operator to mind several machines at once, so that there is a greater risk of output being reduced by having two machines running badly or stopped at the same time.

The trend towards full automation of a complete production line will tend to bring about centralised control, as in the Russian piston factory mentioned by Dr. Mackworth in the paper to which I have already referred. In operating aircraft also there is centralised control with information about the performance of the machine brought to a panel of dials relatively close to one another, but I shall not enlarge on these examples as my colleagues have studied such situations and I have not.

The types of signal or indications of the existing situation may also vary, and the consequences of the operator failing to take action may vary with them. First, there is the evanescent or brief signal such as that appearing on the radar screen. If the signal is missed, no action is possible and the target, submarine, or whatever it is, is not detected. In this situation there is no control over the signal by the operator. In industry there are similar situations, and these usually occur in tasks which are machine paced, so that the rate of working is not controlled by the operator.

Secondly, there are signals which persist until the operator takes action. In this category we can include such happenings as machines breaking down or running out of adjustment, or machines requiring feeding. Consequences may or may not be serious. It probably does not matter very much if a machine is allowed to use up all the materials supplied to it and then runs on doing no work, or comes to a stop until given more work; but if the motorist ignores his oil-pressure warning light or a low reading on his gauge he may seize his engine; or if he ignores his battery discharge light he may run his battery flat.

Thirdly, there are signals over which the operator has some degree of control. With this type of signal the operator is pacing his own task and can search the display for a persistent signal for as long as he wishes. Under this heading come

probably the major proportion of inspection tasks in industry; and it is with this type of task I intend to deal in greater detail. Usually these tasks involve the serial inspection of articles, and within reason the operator can take as long over each article as he wishes, although he may at times be forced on a little by a foreman. If he suspects that something is amiss he can have a closer look and the signal does not disappear before he does so.

Although inspection of this sort is so very common, it has received remarkably little attention, at least judging by published research. I suspect that there is some unpublished work, and that it has remained unpublished because many firms are sensitive about admitting to the amount of rejected work produced. Of course, it could be maintained that if rejections are high, then the firm could say that its inspection is efficient, or thorough, or painstaking, and if low, then it could be maintained that the quality of the articles is consistently high.

Different inspection tasks involve different senses. The gustatory and olfactory senses are used in tasting samples of food; the tactile sense is used in grading of furs; the auditory sense is used in testing railway wheels for cracks; the kinaesthetic or vibratory sense is used in testing small ball races which are held by the fingers over a rotating spindle; and finally there is the visual sense which is probably most commonly used, and which has been most studied, although, as I have said, not studied nearly enough so far.

Now inspection can require yes/no type responses, or there may be some additional complication introduced by a requirement of decision-taking after the signal has been detected. In some of my own work which I will describe later, there is a first requirement to detect impurities in glass ampoules filled with a clear liquid, and then a further requirement to decide whether the impurities thus detected are small particles of glass or bits of fluff or dust. Other tasks require grading of articles into several categories, one can think here of grading of natural products such as timber, and, in Norway, of dried cod.

Such grading involves some form of subjective judgment, and attempts have been made, with varying degrees of success, to eliminate this. I think that most students of psychology have heard the story of the man who judged the temperature of molten metal, I think it was, by spitting into the middle of it and estimating the time it took to sizzle away. First attempts at measuring the temperature by means of a thermometer failed, because the temperature was taken at the edge of the liquid and not in the centre where the spit had been directed. (Omelette pans, I am told, are sometimes tested for temperature by spitting on them.) It has also been shown by Udny Yule (1927) that scales are often inaccurately read because persons have preferences for certain numbers, and because scales tend to emphasise certain divisions; halves, quarters, and so on. If any of you have a series of readings taken off a slide-rule or similar scale and count the numbers of each of the final digits, you will likely find that some digits occur more often than others, whereas obviously they should all occur with equal frequency, if you were using the decimal system at the time.

It will, I hope, be apparent that visual inspection is complicated by factors of vigilance, subjective judgment and decision taking. There is, therefore, a very real need to carry out further research into inspection tasks, with special reference to defining the relative importance of these factors. If at times it seems that the accounts of researches which I will describe are a bit muddled, I can only plead that the various factors I have been describing are all complicating the issue, and the researches do not always make clear which are operating.

Most of the research on inspection has been concerned with the goodness or validity of the inspection. The causes of failure to achieve good results have not been studied to any great extent, but there have been a few attempts to study the effect of altering the inspection situation. The greatest number of studies have

been in the medical field, and most of these have been concerned with the reading of X-ray plates or films.

Bakwin (1945) cites an experiment on the examination of throats of 1,000 eleven-year-old school children. On the first examination it was found that 611 of these children had already had their tonsils removed; while a further 174 were recommended for operations. This figure with the 611 already subjected to tonsillectomy, gives a total of 785 children. The remaining children were examined by different medical men, who recommended that 99 of them should have their tonsils out, bringing the total to 884. A third examination of the remainder by different doctors again added 51 to the total, so that of the original 1,000 children only 65 remained who had neither had their tonsils removed nor had been recommended for tonsillectomy. This experiment was only discontinued because the supply of doctors ran out.

The bulk of the published work, however, deals with reading of X-ray plates, radiographs, photofluorograms, etc. In this country most of the work has been carried out by Fletcher (1954) and Fletcher and Oldham (1949, 1951), and in the U.S. by Yerushalmy (1950, 1951), Birkelo (1947), Garland (1950), Zwerling (1951) and their co-workers, and in Denmark by Groth-Petersen, Løvgreen and Thilleman (1952). The British and American researches combined forces for one study. The results of all these studies are clear and in good agreement with each other. (*a*) It was demonstrated that interpretations of the X-rays differed widely from person to person; (*b*) any one person would be liable to alter his interpretation on a second reading of the same material; (*c*) the actual size and type of X-ray made little difference to the results; (*d*) the common industrial practice of providing the inspectors with standards (in this instance films), reduced the variability in interpretation; (*e*) preliminary set or expectation affected the results so that an 'optimistic' attitude produced more missed positives than a 'pessimistic' attitude; (*f*) the types of classification required affected the results. In general, a small number of classes or categories produces better results.

From these results the authors favour some form of double reading of the films, but admit that this will not eliminate possible injustice caused by wrongly diagnosing those with symptoms, nor anxiety in those persons read as false positives.

With regard to industry itself, there has not been very much research on vigilance. Some of the older studies on tasks involving vigilance, such as spinning, were carried out before vigilance itself had been studied systematically, and therefore the orientation of the research is towards such factors as 'fatigue', and 'boredom'. The literature on inspection is also sparse. Unreliability or errors in inspection are often put down to carelessness, cf. the recent coroner's inquest on a car smash caused by a worn and fractured steering tie rod on a car recently passed as serviceable by the Ministry of Transport inspection station at Hendon. In spite of such happenings, old cars are to be put at the mercy of commercial inspectors. Again, a Select Committee on Estimates on 'R.A.F. Non-flying Establishments' say in their report: 'Your Committee stress again the need for maintenance of a high standard of inspection, which should be possible without duplication'. As Broadbent said in his paper, moralising does not get us far. I suggest that this is an area for the scientist, not for a committee of politicians, or any other sort of committee for that matter.

Most of the studies on inspection have been unsatisfactory for one reason or another. The chief failings have been the lack of quantitative information regarding the work; duration of time spent on the task, with rest pauses; learning time; the frequency of appearance of rejects; and the criterion measure of what is good and what bad. The paper by Raphael (1953) at least in its printed form, mentions the frequency of occurrence of faulty jam jars in a factory with three inspection points. This proved to be one in 10,000 and occurred roughly every hour while the jars

passed along the production line. Thus there is in this situation an infrequent signal, and errors or missed signals are therefore likely to occur. In this instance the customers' complaints of faulty jars were one in one million, but the British are not always prone to complain to the makers of faulty products, so the real number or proportion of faulty jars may be higher than this.

The problem of 'accuracy' of inspection is a very difficult one. If inspection is accurate then there is no problem, if it is not, how do we set about measuring accuracy ? In the radar situation it is possible to construct a simulator and deliberately insert signals. This approach is feasible in some industrial situations, or in laboratory experiments based on industrial processes, but in others it is difficult to imagine a solution along these lines. My own researches have been concerned with the inspection of 1 cc. glass ampoules filled with a clear liquid. The troublesome impurities are small fragments of glass or little bits of hair or dust. These impurities are minute and difficult to detect, even under ideal conditions. They are also difficult to insert in the ampoules deliberately, i.e. to give samples of known impurity.

My original interest in inspection was chiefly concerned with the effects of batching, i.e. with different sizes of lots, or boxes, or quantities of work presented to the operator at any given time, or in some instances, completed by the operator. It had been shown long ago by Harding and Manning (1928–9) that the output of fruit-pickers could be doubled over the day by halving the size of the basket which they had to fill at any one time, and I found evidence a few years ago of rejection rate depending on batch size (Irvine, 1951). The work I will now describe was originally intended to be an extension of that earlier work, but it inevitably involved a study of many other different factors as well, including the problem of accuracy of inspection.

For the main experiment 24 subjects inspected the same 200 ampoules on four occasions, twice as batches of 100 and twice as a single batch of 200. The ampoules were numbered by small tabs round their necks, but the subjects could not see the numbers as they were on the inside and the ampoules were picked up by the neck. Rejected ampoules had to be classified according to whether the subject thought they contained pieces of fluff or dust, or fragments of glass. Good or pure ampoules were placed in a separate box. Times of inspection were taken, and the subjects (or the 11 available when I had the apparatus) were given vision tests. All subjects also were given a checking test. Each ampoule was thus inspected on 96 occasions by the subjects, and in addition each ampoule was inspected 32 times by myself. Analysis of variance and correlational analysis were applied to the data.

This analysis showed, as might be expected, that there were very great differences between persons. It was also shown that the rejection rate depended on the batch size. This batch size effect was statistically significant taken for rejection according to glass fragments by themselves, and the interaction effects between the persons and batch size were statistically significant for rejections classified as pieces, glass, and both together. This shows that batch sizes affect different people in different ways.

Reliability of the inspection proved a difficult problem. On the assumption that if an ampoule is frequently rejected for one type of impurity, then it should seldom be rejected for the other, correlation coefficients were calculated for both boxes used in the experiment on the 24 subjects. Neither was significant. However, using the same argument and calculations on my own data, one of the coefficients ($-0{\cdot}2565$) was found to be significant at the $0{\cdot}01$ level. The subjects' reject rate was correlated with that of the author, taking first pieces and then glass for each of the boxes of 100 ampoules used. All four correlation coefficients were highly significant, and three of them were large, being over $0{\cdot}8$. This finding suggests that reliability of inspection is high only when carried out many times. The final

total scores appear to grade the ampoules according to goodness—i.e. a grading according to ease of detection.

Of the other findings I think that the following are worthy of mention here:

(1) The batch size effect is attributable to both boxes of 100 taken together and not to any one taken separately—i.e. it is not due to just one box by itself.

(2) There is, however, a difference in time taken to inspect the two batches of 100. The first batch had an average time of 14·30 seconds and the second 12·97.

(3) There were great differences between persons according to times taken, but no correlation between times taken and reject rate. A person who took a long time was not better than one who did not.

(4) Perhaps because of the small number of persons available at the time when a vision testing apparatus could be borrowed, relationships between various test scores and rejects were not clearly established, except that it was found that those with poor acuity tended to reject more ampoules than the others. They appeared to be seeing more than those with good vision.

Summary

Already many industrial tasks have a large vigilance component in them. The tasks are mainly in two categories, (*a*) machine minding, and (*b*) inspection. There is every indication that these types of task will tend more and more to predominate over tasks of assembly, tasks of feeding and clearing the machines, and simple manipulative tasks.

Vigilance tasks in the factory of the machine minding type vary from relatively simple ones of watching one machine to watching several, or else watching the indicators from several. There is the likelihood that attempts will be made to centralise control of production as in the Russian piston factory, and thus the vigilance tasks will tend towards greater complexity.

It has also been shown that the warning signals are of different type, i.e. in brief, of varying frequency, intensity and duration. The consequences of failure to notice the signal and taking the necessary action also vary.

Inspection tasks may involve any of the senses, but visual inspection is the commonest type and has received most attention, although still not enough.

Inspection may require a simple yes/no type of response or may require some more complex decision taking. In addition, subjective judgment may be called for, and further difficulty may arise by subjects' preferences for final digits when reading scales.

There is evidence from the published literature that in inspection tasks there are great differences between persons both with regard to accuracy of the work and speed of working. The accuracy tends to be poor for a single inspection, and this applies to medical diagnosis—as far as we have valid evidence. There is also evidence that varying the size of batch also affects the inspection rejection rate for one task studied. I have commented briefly on the other subordinate findings.

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SOME THEORETICAL AND PRACTICAL IMPLICATIONS ARISING FROM THE SYMPOSIUM

DISCUSSION—I

By D. Wallis

You will recall Dr. Mackworth's analogy of the house called 'Vigilance', and will, I think, agree that the morning's papers have thrown considerable light on some of its rooms. Visitors may still find it rather dark, however, when crossing from one room to another and it might be helpful to indicate those passages which show promise of being better illuminated. I will call them Theory, Methodology, Experimental Results and Practical Issues.

Although no-one would claim that a complete theory of vigilance is yet possible, progress is evidently being made towards one. Dr. Mackworth and Mr. Broadbent have shown that behaviour in a wide range of 'active' as well as 'passive' tasks can now be studied within the framework of vigilance concepts. There is a clearer appreciation of the main environmental variables involved, such as signal frequency and duration of watch and their relation to constructs like 'expectancy' and 'reinforcement'. Eventually vigilance may be integrated with behaviour theory generally. Several years ago, Berlyne suggested that attention might be regarded as the 'momentary effective reaction-potential' ${}_{S}(E)_{R}$ of the perceptual response.

Considerable advance has been demonstrated in methodology, particularly where measures of performance are concerned. Mr. Broadbent has stressed the need to use criteria which will reveal variability and brief lapses of attention, as well as the average efficiency during successive periods. The technique described by Mr. Elliott affords the chance, for the first time in this country, of sampling the precise level of vigilance maintained by a subject throughout lengthy periods. I envisage a wide application for this technique. It could be used, for example, in experiments to distinguish between the effectiveness of 'expectancy' or 'reinforcement' in explaining some recent experimental results; Mr. Elliott's method will eliminate the need in this type of experiment of distinguishing 'signals given' from 'signals received' by the subject.

Many experimental results have been cited and discussed by the four speakers, and a number of facts about performance in vigilance tasks seem to be reasonably well-established. There remain, inevitably, points of disagreement on their interpretation; for example, the recent findings quoted by Dr. Mackworth that in one experiment the percentage of signals detected rose from 45 per cent to 90 per cent, when signal frequency was raised from 10 to 40 per hour. This result reinforces our evidence that signal frequency is a major determinant of vigilance. However, it leaves the conviction also that increasing frequency will not necessarily remove the

brief but serious lapses which always occur occasionally from an otherwise adequate level of alertness.

Dr. Irvine's paper has illustrated the importance of vigilance to the success of many inspection tasks. Clearly, however, there are other factors also which influence them. It seems reasonable to judge that some practical aid could be given to improve inspection efficiency, though each task will need careful study. Knowledge of how signal frequency and inter-signal intervals, pacing and batch size affect vigilance should be applicable to inspection problems. At present, I think that only the use of more than one 'inspector' for the same task can get over the problem of the occasional serious lapse from vigilance. In a recent study I found that detection probabilities for visual signals could be doubled by this means, since the lapses suffered by two individuals were uncorrelated in time.

Finally, from observations made during this same study, of apparent differences in temperament betrayed by subjects during long watch-keeping spells, it is not clear whether such differences are directly related to performance in these situations. I think, however, that further investigation of personality factors might prove of some significance for the understanding of vigilant performance.

Discussion—II

By Dr. D. C. Fraser

To continue the analogy with a house, this contribution represents a corridor compressed into a cupboard.

Rather too much has been made of the differences in experimental findings of different investigators. It is essential to distinguish between 'vigilance' which has the status of an intervening variable, and a 'vigilance task', which is a clearly defined experimental arrangement characterised by the following three properties: (*a*) the display consists of a series of neutral signals throughout which the significant signals are randomly interspersed; (*b*) the conditions of the experiment are such as to render it a stress situation, in terms of speed, load, duration, etc.; (*c*) knowledge of results is minimal.

If, and only if, these three conditions are fulfilled can we expect the findings of the classical vigilance situation to apply, and in such a case there is a considerable measure of agreement in the experimental findings. But experimental arrangements like those of Mr. Elliott or Dr. Bakan, in which an undetected signal is presented again at higher strength, are not true vigilance tasks, and should not be expected to give comparable results. This is not in any sense a criticism of their very interesting techniques.

Again, it is commonly assumed that breakdown in vigilance is of the same kind whatever the watch-keeping situation. But some recent experiments which we have just published strongly suggest that there are at least two patterns of breakdown, and therefore, possibly, two kinds of vigilance: (*a*) the alertness needed to detect a very occasional signal occurring among slowly presented neutral signals over a long period of time; (*b*) the alertness required to detect an occasional signal appearing among many neutral signals of short duration over a comparatively short period of time.

I find myself agreeing with both Mr. Broadbent and Mr. Elliott in many respects. I agree with Mr. Broadbent that many active tasks involve a strong vigilance component, and consider his analysis of individual differences of great importance. I think that Mr. Elliott's empirical curves are capable of giving a good approximation to the true situation, but his analysis might benefit from the consideration of time of day effects, which seem to be quite marked in some people, and from a fuller consideration of individual differences.

Mr. Elliott's emphasis on a more lifelike approach to vigilance testing is to be welcomed; but it is perhaps more likely to be achieved by actual sampling from the

job itself than by effective threshold techniques. Recent experiments of this kind at Farnborough have shown that accuracy of pilot performance in flying real aircraft, under conditions which have some of the characteristics of the classical vigilance situation, may fall off after 40 minutes of continuous flying—which is very close to Dr. Mackworth's original findings. There was also a further and more acute fall-off towards the end of a long flight.

It is perhaps worth while just mentioning the usefulness of the vigilance task as a measure of stress and fatigue. In our experiments it has proved more successful than any other test as an index of fatigue as the result of prolonged flying, sleeplessness, exposure to heat, and even, as Mr. Broadbent has demonstrated, exposure to high ambient noise.

The two greatest needs now, in my opinion, are (1) a comprehensive theoretical framework to explain all the differing manifestations of vigilance (and for this purpose it might be a good thing if different investigators would agree to use the same or comparable forms of vigilance task) and (2) a full study of the physiological correlates of vigilance. An attempt has been made to do this in Farnborough, and in some cases it has been possible to demonstrate a strikingly close correlation. In view of the possibility that we are touching here on some fundamental properties of the nervous system, it would be intriguing to correlate vigilance performance with neurophysiological measures: preliminary arrangements have been made with Professor Drever to use his technique to study changes in the neurophysiological pattern during the performance of vigilance tasks. It may be that such an investigation would be more fruitful than previous attempts to correlate neurophysiological data with subjective reports.

Discussion—III

By Dr. P. Bakan

The occurrence of performance decrement over time in vigilance tasks has been shown to be the result of an increase in the effective threshold for the discrimination required by the task (Bakan, 1955). There is evidence to indicate that a loss of sensitivity in vigilance tasks is related to a tendency toward drowsiness and sleep. Variables related to sleep seem also to be related to performance in vigilance tasks. Monotonous environments lead to both sleep and vigilance decrements. The drug benzedrine leads to both wakefulness and prevention of decrement. Subjects who show decrements in a vigilance task also show changes in E.E.G. patterns characteristic of a drowsy state. A diminution of skeletal muscle action potentials is characteristic of sleep and occurs when vigilance is low. Despite the existence of these parallels, subjects in vigilance tasks don't usually fall asleep. There seems to develop a conflict between the tendency to fall asleep and a desire to remain awake to do the job. Subjects may resolve this conflict by counteracting the monotony of the environment with forms of self-stimulation such as moving about, singing or whistling, or day-dreaming and mind-wandering. These activities may be detrimental to the performance of the vigilance tasks. Attention to one's thoughts, for example, may detract from attention to critical stimuli in the external environment. Some self-stimulating activities may be more detrimental than others. Further analysis of what subjects actually do during the vigil, and how these activities are related to performance might supply us with fuller understanding of vigilance behaviour. It might be possible to encourage subjects to engage in forms of self-stimulation which are not detrimental to performance, or for the experimenter to introduce stimulation irrelevant to the particular vigilance task and yet compatible with it.

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POLAR PHYSIOLOGY

SECTION I (PHYSIOLOGY) devoted the morning of Tuesday, September 4, 1956, at the Sheffield Meeting of the British Association to a discussion of 'Polar Physiology'. Dr. O. G. Edholm opened the symposium with a paper which indicated that valuable studies of the long-term effects of a cold environment can be made on the members of a polar expedition, because of their isolation and training. He was followed by Dr. H. E. Lewis, whose contribution dealt with sleep and wakefulness patterns in men on a polar expedition. Dr. Mary Lobban then reviewed certain studies on physiological diurnal rhythms in polar regions. Dr. J. P. Masterton presented a paper on nutrition and energy expenditure during a polar expedition. Mr. R. J. F. Taylor, in a study of sledge dogs, described the old diet of huskies and the production of a new sledging ration. Dr. Paul Massey then presented a paper on acclimatisation to cold, in which he described certain finger-numbing experiments. The final contribution, from Dr. A. F. Rogers, was concerned with physiological investigations to be undertaken on the forthcoming Transantarctic Expedition.

Abbreviated versions of the contributions to this session are printed below in the order in which they were given.

POLAR PHYSIOLOGY

By Dr. O. G. Edholm

(*Head of the Division of Human Physiology, Medical Research Council*)

The first question to be asked is 'What is polar physiology?' A bald answer would be a study of the physiological effects on man of life in polar regions. Essentially, it is the effects of the environment with which we are concerned. The peculiarities of the polar environment are the low temperatures and the alternation of light and darkness. The effect of cold on man has been the object of many physiological investigations. It is by no means certain that man does adapt to cold in the way that animals do. Experiments have been carried out in cold chambers, but it is difficult to do such experiments for long periods. So the obvious method is to study people who live in cold climates.

Members of a polar expedition are ideal in many ways for such enquiries. They can be studied in a temperate climate, before they set out, and can be studied during the course of the expedition, to find out what changes they exhibit.

The long polar night and equally long polar day also may produce changes in what are termed diurnal rhythms. There are a considerable number of biological rhythms which occur in the course of the normal day. Body temperature rises during the day and falls at night; there are variations in the renal rhythm with increased secretion of urine during the day and a marked fall at night. It is of considerable interest to know how such rhythms are affected by a different pattern of darkness and light.

The Climatic Physiology Committee of the Medical Research Council has been the main body concerned in Britain with the initiation of physiological work in polar regions. When the British North Greenland Expedition was being organised, it was agreed that a physiologist (Dr. H. E. Lewis) should accompany the expedition.

The principal subjects studied and measurements made were: (1) basal metabolic rate; (2) weight changes and changes in subcutaneous fat thickness; (3) food intakes; (4) sleep rhythms; (5) diurnal temperatures; (6) fat content of faeces; (7) studies on blood, including (*a*) haematocrit, (*b*) cell counts, (*c*) fragility, (*d*) haemoglobin.

There have been two expeditions to Spitzbergen sponsored by the Climatic Physiology Committee and financed by the Medical Research Council. These expeditions have been led by Dr. Mary Lobban, the prime object being to study diurnal rhythms in man under conditions of continuous daylight in the summer months in Spitzbergen. On the first expedition the members lived a 22-hour day for several weeks; on the second expedition there were two groups, one living a 21-hour day and the other a 27-hour day.

Amongst the stated objects of the British Transantarctic Expedition is the study of polar physiology, and there has been close collaboration between the leader of the Expedition, Dr. V. E. Fuchs, and the Climatic Physiology Committee. Dr. Alan Rogers, Lecturer in Physiology at the University of Bristol, has been selected as the physiologist, and he will also act as medical officer. His programme of research has been drawn up after discussion with the Climatic Physiology Committee.

There are also other medical officers who have been briefed on suitable problems for study in polar regions. These include Dr. Goldsmith, the medical officer of the advance party of the Transantarctic Expedition. This party is now building its base at Vahsel Bay in the Weddell Sea. Surgeon-Commander Dalgleish, the leader of the Royal Society party who have their base at Coats Land, hopes to make some physiological observations. There are several medical officers with the Falkland Islands Dependencies Survey who have been equipped and briefed.

It has been decided that the most fruitful line of work is to extend the observations made by Dr. Lewis and Dr. Masterton in Greenland. They showed that there was a considerable change in body-weight, increasing in the winter, diminishing in the summer, and associated with this change in weight there was an increase in the thickness of subcutaneous fat. All those mentioned above have therefore been equipped with standard callipers for measuring subcutaneous fat thickness and will be recording this, together with body-weight, at regular intervals on the various members of the expedition.

SLEEP AND WAKEFULNESS PATTERNS IN MEN ON A POLAR EXPEDITION

By Dr. H. E. Lewis and Dr. J. P. Masterton
(*British North Greenland Expedition* 1952–1954)
*Medical Research Council, Hampstead, London, N.W.*3.

One's sleeping habits tend to be largely governed by the diurnal cycle of the sun. But there are two parts of the globe—the polar circles—where the periods of light and darkness are not diurnal but seasonal, viz. there is a three-monthly summer period of continuous sunlight and a three-monthly period of winter darkness.

What is the effect of the day-night seasonal pattern on sleep? What time is sleep taken? And if there are no restrictions in the amount of sleep taken—viz. 'demand sleeping', cf. 'demand feeding'—how many hours do we sleep?

There was an opportunity to study these problems on the British North Greenland Expedition. The object of this polar expedition was to explore unknown territory near the North Pole, and it comprised twenty-five members who were away for one to two years. The expedition was established at a latitude of about 78° N., well within the Arctic Circle, less than 800 miles from the North Pole. We did not live as a strictly organised community. Generally speaking, as long as the scientific programme or the domestic work of the day was done there was no reason why one should not sleep at any time during the 24 hours.

A record of sleep was made in this way: each man was issued with a specially printed sleep card which he filled in when he came to write his diary every day. At the end of each month the completed card was exchanged for a new one.

In North Greenland there are sunrises and sunsets for September and October,

but by November the sun does not rise above the horizon and one is in complete darkness for three months. The light returns about February, and until April there are sunsets and sunrises, but after that the sun is in the heavens for 24 hours of the day.

When the cards were analysed it was seen how the effect of the winter darkness severely disrupted the usual pattern of sleep—say midnight to 8 a.m. The disruption in most members was marked by naps and periods of interrupted sleep; subsequently, in the spring, there was a return to the more usual pattern but this was once again disturbed during the summer characterised by the midnight sun. There was no relationship between interruptions of sleep and naps taken. It was also seen how individuals, whose social and disciplinary background was similar, varied enormously in their sleeping habits.

When the monthly sleep was totalled there was little difference over the two years. During the dark period members were going to bed and taking naps at all times over the 24 hours, and one was given the impression that they were sleeping excessively. This was not so. In considering the total monthly sleep, the mean for the members was 7·9 hours, which is quite interesting since they were at liberty to sleep practically as long as they liked, whenever they liked. The approximate period of 8 hours' sleep taken in temperate countries would indeed appear to represent a normal demand.

STUDIES ON PHYSIOLOGICAL DIURNAL RHYTHMS IN POLAR REGIONS

By Dr. Mary C. Lobban

(*Physiological Laboratory, University of Cambridge*)

In the summers of 1953 and 1955, two expeditions have gone out from Cambridge to Spitzbergen to study the effects of prolonged periods of life on abnormal time routines upon physiological diurnal rhythms in man. In Spitzbergen the climatic conditions are such that a very high latitude can be reached with comparative ease in the summer months, and human subjects can be maintained there for considerable periods of time in isolation and under conditions of almost constant daylight and environmental temperature.

In 1953 eight human subjects lived for six weeks on a 22-hour day, while in 1955 one group of seven subjects lived on a 21-hour day and another group of five subjects lived on a 27-hour day for a similar period. In all three groups urine samples were collected from the subjects at frequent intervals on experimental recording days and a complete study was made of their diurnal excretory rhythms throughout the experimental period. On these recording days, diet, fluid intake and all physical activities were carefully controlled. During the 1955 expedition the diurnal variations in body temperatures were also recorded.

From the results obtained on the two expeditions it would seem that rapid adaptation of the urinary rhythm to an abnormal time routine is an uncommon occurrence. In nearly all the subjects on all three time-scales the intrinsic 24-hour excretory rhythm persisted to a greater or lesser degree, and in some it was maintained unimpaired for the whole duration of the experiment. Subjects who failed to adapt to the environmental time routine showed signs of stress, particularly at those times when their excretory rhythms became greatly disorganised. In at least three subjects on the 1955 expedition the disorganisation of their excretory rhythms was peculiarly complex, in that a complete dissociation occurred between the rhythms of the excretion of water and of potassium. Further evidence for the dissociation of physiological rhythms was provided by the body temperature recordings: in all subjects on both 21-hour and 27-hour routines the body temperature rhythm appeared to adapt rapidly and completely to the environmental routine, irrespective of the degree of adaptation manifested by the excretory rhythms.

These experiments provide evidence for the persistence of the intrinsic, 24-hour rhythm of kidney function in some human subjects: they also indicate that the mechanism which controls the normal diurnal variations in body temperature is not the same as that controlling the excretory rhythm.

NUTRITION AND ENERGY EXPENDITURE DURING A POLAR EXPEDITION

By Dr. J. P. Masterton and Dr. H. E. Lewis (of the Division of Human Physiology, Medical Research Council Labs., Hampstead) and Dr. E. M. Widdowson (Dept. of Experimental Medicine, Cambridge University)

Food intake on a polar expedition has always been a great problem to those in charge of the commissariat. For years it has been accepted that man has a very high energy expenditure while sledging. Because of this it has always been the custom to provide a sledging diet which yields the greatest number of calories in the smallest weight.

In the days of Scott and Shackleton this was not easy because of the limited payload on dog sledges, which had to carry tents, scientific equipment, clothing, fuel and dog food, as well as man food. However, in recent years a ration has been evolved which for a minimum of weight provides an intake of approximately 4,200 calories in the ratio of Protein : Fat : Carbohydrate of 1 : 2·8 : 3·1. This ration weighs 760 g. without packing.

On the British North Greenland Expedition (Lewis & Masterton 1955) which spent two years within 800 miles of the North Pole, there was an opportunity to investigate both the energy intake and expenditure of men doing hard sledging. Many dog-sledge journeys were made over rough glacier country at temperatures around minus 20° C. and it was on such journeys that part of the investigation was carried out. As a control, energy balance studies were made at the Base Hut where we lived throughout the Arctic winter.

Three problems were studied:

(1) The energy balance of men living at the Base Hut.
(2) The energy balance of men while sledging.
(3) The fat utilisation of the body while living on the high fat diet of sledging.

This latter investigation was the one which prompted the whole experiment. We had noted early in the expedition that our husky dogs ate human faeces voraciously. We wondered, since we were living on a high fat diet whether the dogs were deriving any benefit from the faeces in the form of unabsorbed fat.

Four men were chosen for the investigation and three energy balance surveys were carried out—two at the Base Hut and one while sledging.

There was no means of measuring the oxygen consumption, and therefore the energy expenditure, of the men during various activities. The only exact measure we had in this respect was a measure of the basal metabolic rate of each man throughout the three surveys. However, a time-and-motion study of each man's daily activity was made. These time figures were then converted into energy expenditure by comparing them with the mean of many figures for equivalent activities found in the literature (Passmore & Durnin, 1955). From this a figure for each man's daily energy expenditure was found.

The food intake on all the surveys was accurately weighed and the weights of foods were then converted into their constituent items and expressed as protein, fat, carbohydrate and calories using the tables of McCance and Widdowson (1946).

At the Base Hut the daily individual intake for each of the four men was measured for two periods of a week with a four-week interval between each survey.

Normally the sledging ration carried was a standard one which occasionally left one hungry after it had been eaten. Therefore on the sledging survey liberal supplies of extras were taken so that each subject could eat as much as he liked. As a result there was day-to-day and inter-subject variation between the sledging dietary intake. The sledging survey was made on the same four men, who sledged in two parties of two. While they were sledging the temperatures were around minus 20° C. and the men were doing hard sledging or rock scrambling for 6–8 hours per day with occasional rest days. The experiment lasted nineteen days in the case of one party and ten days in the case of the other. In each the men had been out for a week or more before the investigation began.

From two of the men the total stools were collected during fourteen days while the energy balance survey was in progress. These stools froze very soon after they were passed and were then scooped into polythene bags in which they were stored. When the thaw came they were stored with chipped ice in large thermos flasks. Finally they were brought back to Britain in the frozen state where they were analysed for their fat content.

The four subjects were normal healthy adults. Table I shows their ages, heights and average weights during the surveys.

TABLE I

Ages, Heights and Average Weights of Subjects

Name	Age, yrs.	Height, cm.	Av. Weight, kg.
R. B.	26	176	77
J. M.	25	166	70
A. E.	25	182	83
P. W.	23	180	87

It was found that while living at the Base Hut there was considerable variation in the food intakes and also in the estimate of the energy expenditure. The intake ranged from 1,630 to 8,760 calories per day with a mean of 3,880 calories.

While sledging the intake was much more constant. This may have been due to the fact that the energy expenditure was more uniform in that each day's activity consisted of 6–8 hours' sledging and 16–18 hours in the tents. On the other hand, at Base, activities varied from sitting writing all day to ski-ing and other hard exercise for a number of hours.

While sledging the intakes ranged from 4,000 calories to 5,740 calories per day with a mean of 4,770 calories per day. Table II shows the mean daily energy balance of the four subjects on all three surveys.

TABLE II

Mean Daily Intake and Expenditure on Surveys

	R. B.		J. M.		A. E.		P. W.	
	Intake	Expenditure	Intake	Expenditure	Intake	Expenditure	Intake	Expenditure
Base I	3,300	3,706	3,600	3,442	3,600	3,778	5,090	3,618
Base II	3,780	3,310	3,570	3,666	3,910	3,563	4,440	3,566
Sledging	4,850	5,119	4,520	5,013	4,870	5,311	4,840	5,349

It was found that the fat intake while sledging was high and for the two subjects from which the total stools were collected for fourteen days the mean fat intake was 285 g. and 273 g. respectively.

Table III shows the composition of the faeces of the two men and the intakes and absorptions of fat on this high fat diet. In spite of an intake of the order of 280 g. of fat per day these men had an absorption of 96·8 per cent and 96·3 per cent.

TABLE III

Composition of Faeces of Two Men

(Mean of 14 days)

Weight of fresh faeces g./day	164·0	162·0
Weight of dry faeces g./day	39·3	40·5
Fat g./100 g. dry faeces	22·3	23·7
Intake of fat g./day	285·0	273·0
Excretion in faeces g./day	9·0	10·0
Absorption g./day	276·0	263·0
Absorption as per cent intake	96·8	96·3

Therefore the dogs were certainly not deriving any significant quantity of nutrition from the faeces and unless future investigations can show the presence of some other substance in the faeces useful to dogs, we must assume that they ate human faeces because they liked it.

Finally the figure of 4,770 calories for the mean calorie intake while sledging would suggest that previous sledging rations offering 4,200 calories is rather low and future expeditions should aim at offering a ration which gives 4,800 calories per day. With modern packaging techniques this new ration should weigh no more than the old ration packed in tins as it has been until the present.

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SOME ASPECTS OF THE PHYSIOLOGY OF SLEDGE DOGS

By R. J. F. Taylor

(*Falkland Islands Dependencies Survey and the Department of Zoology, Cambridge*)

In 1954–55 an investigation was made into the work output of dogs under Antarctic conditions. This depended on the measurement of the force exerted by a team of dogs pulling a sledge. The pull was measured electrically, using a new type of strain gauge. The pull exerted altered the electrical resistance of the gauges and this change was measured by a milliameter mounted at the back of the sledge. There are extensive records from the use of this apparatus and the results are being presented in various journals.

Work is the product of force and distance, and so load plays a dominant part in determining work output. With no load, no 'useful' work is done however fast the team moves. With too heavy a load there is no work output as the dogs cannot move the sledge. At some intermediate load or loads the work output will be a maximum. The maximum speed of a particular team, unladen, was around 15 m.p.h. and the maximum pull they could exert to keep a heavy sledge moving was just over 300 lb.

wt. The maximum work output occurred, when trotting at 5·0 m.p.h. with a drag of 120 lb. wt., and when walking at 3·2 m.p.h. with a drag of 200 lb. wt. The maximum 'useful' work output, from the team of nine dogs of average weight 85 lb., was 1,100 calories during one hour. For a few minutes the work output can be increased by 23 per cent. These figures for the maximum work done by the dogs are similar, weight for weight, to that produced by the rowing eight that won their event in the Olympic Games of 1924.

It is suggested that different factors may limit the maximum work output at different loads. The suggestions are that the rate of oxygen supply to the muscles limits useful work output when trotting, and that muscle power limits useful work output when walking. These controls can be likened to fuel supply and to engine size. Dogs can only gallop with an extremely light sledge. While galloping there are instants in time when all the feet are off the ground together, and so the drag will result in alternate retardation and acceleration of the body and therefore energy is wasted. It is suggested that the type of gait, the 'gear box', limits maximum useful work output while galloping with a sledge and not oxygen uptake or total muscle power.

The muscles of two sledge dogs were examined in an attempt to find the total weight of locomotory muscle. It is extremely difficult, from dissections, to decide exactly which muscles are employed, but a figure was obtained for the power/weight ratio of dog muscle which is slightly higher than the accepted figure for mammalian muscle of 17 watts/kg.

On a journey, diet is rigidly controlled and the details are known as completely as the rations can be analysed. Experiments produced a knowledge of the maintenance needs of resting dogs and the measurement of the work output enables the energy input and output to be balanced. Although this calculation does involve certain drastic assumptions, it has been carried out and three conclusions emerged. The first was that the muscles of the dogs have an efficiency of at least 20 per cent, i.e. they convert chemical energy to mechanical energy at this figure. The second conclusion was that under 'normal sledging conditions' three-quarters of the mechanical power produced in the locomotory muscles is used in moving the sledge and the other quarter is used in moving the dogs. The third conclusion was that a drop in temperature of one degree Fahrenheit demands an increase of 0·3 per cent in maintenance needs.

In the work so far described, the dogs were considered as engines. This is too simple a view and the loss of work output on journeys can be attributed to mechanical and to psychological reasons. On a ration of 1 lb. of pemmican, the loss in work output due to mechanical and nutritional deterioration was 5 per cent a week; on 1¼ lb. and vitamin tablets, this figure was 2 per cent. However the actual working rate on journeys varies from day to day and may fall to 50 per cent of the maximum, even with good sledging surfaces. Also there are occasional marked spurts, usually with obvious mental stimuli, and it was concluded that the psychology of the dogs played a bigger part in determining the variations in their performance than their physical condition. Thus during travel on flat and continuous 'sheets' of ice and snow dogs become bored, and changes in the environment, such as a visual objective, will produce a marked increase in their performance. This point is well presented by Winthrop Young in connection with mountaineering: 'Mental distraction is as important as change of movement for the easy performance of sustained physical effort', and 'Far more than any muscular strength or even physical fitness, will power is the dominant force in maintaining normal energy'.

References

Two papers on sledge dogs have appeared in Numbers 55 and 56, 1957, of *The Polar Research*, and others are being published in *The Journal of Physiology* and *The British Journal of Nutrition*.

FINGER ACCLIMATISATION IN GRAHAMLAND

By Dr. P. M. O. Massey

Anywhere on the Antarctic continent presents one of few areas in the world where man may live in subfreezing temperatures for a year, or even two years at a stretch. In this way any changes which may occur due to the cold can be measured to a maximum degree. To offset this, the observer has to adapt his equipment, experimental technique and mental approach to his subjects (who cannot be exchanged for a fresh batch when they become heartily sick of testing) to the rigours of the weather and of long isolation.

Using a method devised by Dr. Mackworth of Cambridge, one aspect of acclimatisation to cold was measured by the determination of a gap between two keen edges of rulers bolted in a V-shape. When the finger was numb with cold only a wider gap could be felt and as the finger re-warmed so did the size of the gap diminish. The warm finger of different subjects detected different-sized gaps and these initial threshold gap readings were subtracted from the resultant numbness produced by chilling. In this way performance of different people was compared. Using a wind tunnel in which the index finger was chilled, the performance of newcomers to the region could be compared with that of the acclimatised group who were spending a second consecutive year in Antarctica.

The first difference was that on arrival in Grahamland, the newcomers had twice as much finger numbness as the second-year men when exposed to the cold within the wind tunnel. Six weeks later, under identical conditions of chill, there was very little difference between the groups in the numbness produced. This showed that an adaptation of the tactile senses of the finger had occurred within the six weeks. This improvement was only recorded in the newcomers and confirmed previous work in Canada.

The weather in this period of adaptation was comparable to a stiff English winter with the temperature range between 24° F. and 32° F. with an average wind of 13 knots. From this it can be inferred that adaptation to cold can occur in the United Kingdom after a prolonged period of around freezing temperatures. When the thaw comes the spring temperatures are regarded as warm when in fact they are only average for that time of the year and usually regarded as cool.

As the winter set in and the temperatures became colder, the two groups behaved as one, and there was no more improvement as far as tactile discrimination was concerned.

Another difference which was noted was the range of finger pad temperatures. This was measured under resting conditions by a constantan thermocouple strapped to the finger pad by adhesive plaster. Skin temperatures for the second-year men were lower than that of the newcomers.

This was contrary to previous work carried out in Canada and in laboratories. When the hut temperature was considered it simplified the issue. Full vasodilatation had taken place in the 70°–80° F. warmth of centrally heated Canadian huts or laboratories. In the sparse warmth (50° F.) of the Grahamland hut, the acclimatised men with more resilient circulations, had reduced the blood flow to the fingers to conserve heat for the vital centres (heart, brain, viscera). The newcomers with less efficient circulations and undeveloped blood shunts, still supplied heat to the cold finger in a wasteful manner comparable to an unlagged pipe. This gave a higher resting finger temperature than the experienced second-year men.

To add further weight to the validity of this observation, after six months' residence in a sub-freezing climate, there was no difference between the groups. In effect, resting finger temperature of the newcomers fell to that of the second year men and for the remainder of the year, finger pad temperatures were the same.

There were then two main changes which took place in the newcomers, but not

in the second-year men. The fingers became less sensitive to the numbing effect of cold winds in six weeks or less. The resting finger temperatures changed after six months. It may be then assumed that adaptation to cold took place in two phases: rapid and slow.

On ten testing occasions throughout the year, drift snow was inducted into the wind tunnel and blown out onto the finger during cold exposure. Under 'cold' conditions the numbness induced was twice as great with drift snow. Under 'very cold' conditions the difference was over six times as great. With the added chill of drift, adaptive mechanisms collapsed to give high numbness scores and in eighteen cases minor frostbite of the finger.

Then numbness with frostbite was over twice that of fingers without frostbite. Both groups had equal condition of chill. The incidence of frostbite produced fourteen cases in the newcomers, yet only four in the hardened second-year men. This was tested statistically and found to be a very significant finding. This would imply that the second-year men had acquired a certain resistance to frostbite by living in Antarctica for over a year and this resistance had been carried over from the previous year.

'Know-how' and experience of the cold are undoubtedly large factors in avoidance of cold injury, but by exposure within an experimental wind tunnel, these factors are eliminated. In fact, one finger is the same as the next in the wind tunnel and only differs in its response to cold by acquired adaptation.

The men were asked to predict whether they were numb or not before withdrawal from the tunnel. All groups gave correct answers 75 per cent of the time. It is thought that there may be a difference between acclimatised and unacclimatised in the correct predictions given, but the result was obscured by the late introduction of the test when group differences had largely disappeared.

Note. My thanks must be recorded to my companions of the Falkland Islands Dependencies Survey who shared the burden of routine testing throughout the year.

THE PHYSIOLOGICAL PROGRAMME FOR THE TRANS-ANTARCTIC EXPEDITION, 1955–58.

By Dr. A. F. Rogers

(*Department of Physiology, University of Bristol*)

Medical Officer and Physiologist to the Trans-Antarctic Expedition

The Trans-Antarctic Expedition

An expedition, under the Patronage of Her Majesty the Queen, has been planned with the object of crossing the Antarctic continent. Antarctica is about the size of Australia and Europe together, and the journey of about 1,800 miles, across almost the narrowest part, will take just under four months.

Advance Party. An advance party left this country on November 14, 1955, and is now setting up Shackleton Base at Vahsel Bay in the Weddell Sea.

Main Party. The main party, led by Dr. Fuchs, and destined to make the actual crossing, left London in November 1956, reached Vahsel Bay early in 1957, and spent the Antarctic winter at Shackleton Base. A physiologist accompanied this party and crossed Antarctica.

The Crossing. The logistics of the crossing are critical. The party will set out in November 1957, towing its food, fuel and shelter with it. The entire route is over unexplored territory, unseen before even by air, with the exception of the region of the South Pole itself. The first 300 miles are expected to be through mountainous country, and the next 1,200 miles, at altitudes of 9,000–10,000 ft., across the

central plateau, with the South Pole mid-way. This will bring them near Mount Albert Markham, where a reception party, led by Sir Edmund Hillary, will meet them with food and fuel and guide them the last 300 miles through the mountain ranges of Victoria Land to the New Zealand base at McMurdo Sound in the Ross Sea, and the combined parties will leave in February 1958 by ship for New Zealand.

Physiological work. This is a unique opportunity for physiological investigations, for while all major expeditions have a medical officer, only two have had a professional physiologist, namely the Everest expedition and the British North Greenland expedition, and on both of these most valuable physiological work was done. The work to be done on this expedition is a direct continuation of the physiological work done on these former expeditions, and has been planned with the generous help of the Medical Research Council. Private firms have also made valuable gifts of apparatus. Very elaborate experiments cannot be done, for the working conditions at base, and especially during the crossing, will be of unusual difficulty, and only one physiologist is going, not a team. Moreover, the physiologist will also have the duties and responsibilities of medical officer and dentist, to say nothing of the veterinary work with the dog teams which is expected to occupy most of his time. Nor will his time be his own during the day for, like the other members of the expedition, he will have to drive a dog team or a vehicle such as 'Sno-cat' or 'Weasel'. No heavy or bulky apparatus can be taken by the physiologist as food and fuel will occupy almost the whole available space on the crossing. In any case, most conventional apparatus would fail under the combination of low temperatures and rough conditions. Shackleton Base had 96 degrees of frost on August 1, 1956.

Proposed Physiological Investigations. The main investigations on the 15 subjects available will be an attempt to make an ENERGY BALANCE. This involves measuring the calorie intake and output. The food intake gives the first of these and entails a careful and detailed record of *all* the food consumed. Output is measured by regularly recording the body-weight, fat thickness, and activities of the subject including the clothing record, sleep record, and meteorological data. The calorie expenditure of the body will be reflected over any period of time in the consumption of oxygen and the production of carbon dioxide (and water vapour). The oxygen consumption is closely proportional to the amount of air breathed, and the CO_2 production can be measured by collecting a representative sample of expired air and storing it in glass ampoules for analysis later.

This was formerly done using large cumbersome Douglas bags, but can now be done by the use of a readily portable apparatus called an I.M.P. or integrating motor pneumotachograph, recently developed by the Medical Research Council. The air breathed is measured by a mask over the nose and mouth, and a small sample representative of the whole collected in a plastic bag from which it is transferred to a sealed glass ampoule for storage, transfer, and analysis. The calorie expenditure figures will be compared with the input figures and a measure of the calorie cost of Antarctic life and work obtained, it is believed, for the first time.

Finally, it is worth emphasising that there is as yet no generally applicable test for general acclimatisation to cold—in fact, the easiest way to test a person is to ask whether they like the cold! A general watch will be kept for any changes that might be useful in this respect. It is known that sensory changes occur in the finger tips during long sledging journeys: these are reversible, and possibly due to skin changes. A photographic record of finger tips will be kept in order to investigate this.

SCIENCE AS PART OF A GENERAL EDUCATION

SECTION L (Education) devoted the morning of Monday, September 3, 1956, to a discussion of the role of science in a general education. Mr. J. A. Ratcliffe, O.B.E., F.R.S., of The Cavendish Laboratory, Cambridge, read a paper entitled 'Can science courses educate?' He was followed by Dr. R. C. Smail, M.B.E., Lecturer in History in the University of Cambridge, whose contribution was entitled 'The combination of arts with science courses in higher education.'

These two papers are printed below.

CAN SCIENCE COURSES EDUCATE?

By J. A. Ratcliffe, O.B.E., F.R.S.

First I must introduce myself. For the past thirty years I have been lecturing on physics and doing some research in that subject. My colleagues and I have turned out physicists who have gone into universities to teach and research, into schools, and into the technical side of governmental and industrial institutions to help with research, development, and production. Some have been very successful, all have been moderately successful, and we have reason to think that our courses have been not too bad for their purpose, that is for the purpose of turning out practising physicists. I am going to criticise them for another purpose later, but for what they set out to do I think they were rather good.

Meanwhile our arts colleagues, in history or classics for example, have been lecturing to students some of whom have used their specialised knowledge as teachers and research workers in their subjects. Others, however, in considerable proportions, have not used their specialised knowledge after they have left the university. These have gone into government administration, into commerce, and into the managerial side of business, often of technical business.

Now these two sets of lectures, on science on the one hand, and on arts subjects on the other, appear to be attended by undergraduates with different ultimate purposes in view. Of course in each there is the object of doing well in the examination, but after that object is achieved (or not, as the case may be) the scientist expects to use the specialist knowledge he has acquired from the lectures. But amongst the arts men there are several who, from the start, do not intend to practise their specialised subject. They realise that only the general background and methods of thought will be of use to them later. This difference in the audiences leads, I believe, to a difference in the lectures. The science lecturers try to produce first-class practising scientists; the arts lecturers feel that they are responsible not only for turning out specialists, but also for providing a general education for a large proportion of their audience who will never be specialists. They can take the view expressed by Quiller-Couch when he said: 'The man we are proud to turn forth from our universities will be remarkable less for something he can take out of his wallet and exhibit for knowledge, than for being something, and that something recognisable for a man of unmistakable intellectual breeding, whose trained judgment we can trust to choose the better and reject the worse.'

It seems to me that there is an important difference between lectures in science and in the arts in this matter of general education for the non-specialist. I want this morning to concentrate attention on that difference, and I should like at the start to define 'general education' at the university as *that part of a university education*

which is of value to a student who afterwards makes no use of the detailed knowledge he has acquired. I have no intention, particularly in Section L of the British Association, of arguing about what the purpose of education really is, I will simply adopt my definition of general education so that you can be clear what I am talking about.

I shall assume that it is a part, and a considerable part, of the duty of a university to provide a general education in this sense. I believe that this kind of education is well provided at present by courses in the arts, my justification for that supposition being that men with arts degrees rise to the highest positions in government, management, and the like, in work where their specialist knowledge has never been directly used. But I believe that the general education we provide would be even better if half of it were provided through science and half through an arts course. That is my thesis this morning.

Now it is arguable whether, or not, science lectures can provide a 'general education' of the kind I am talking about, and I will enter upon that argument shortly, but first I wish to point out that, whether they do or not, there is little doubt that very few students attending them make use of them for that purpose. The overwhelming majority attend because afterwards they hope to practise as scientists. Only a very few use their science simply as a broad educational preparation, after which they can go off and do something quite different. It appears that those who go to the university for a 'general education' read arts and become our administrators and managers; those who read science become our scientists and technicians.

Now this difference raises the important question, 'Why do not more of those who read science at the university read it as a "general education", preparing them to go away and engage in general affairs?'

I think the answer is, quite simply, that science is too useful. A boy interested in science knows that there is plenty of scientific work to be done in the world and that he can go on, practising what he is interested in, as long as he remains interested. He would much rather do that than just study it and then do something quite different. But classics for example, or history, is no use in this sense. However interested you may be in it, you must (unless you are one of the chosen few) be content to use it simply to train your mind, and you know that you will have to do something quite different for your main work. Now a subject of which that is true has a clear advantage when it comes to general education. From this point of view science suffers because it is too useful and because it is so easy for one who reads science to go on doing science.

But there still remains the question why some of those who want a general education do not try to get it through science. The reason, I believe, is that the science courses provided are not suitable. The purpose of these courses is to turn out practising scientists, and the people who go to them will want to be practising scientists. They will have to be interested not only in the broader issues of the subject, but also in those details which the practician will require. In physics, for example, they will spend much time considering special techniques of measurement and special mathematical methods. The position is that the science courses are intended for the future specialist, and are not suitable for those who do not wish to become specialists. As a result they are little, if at all, used in our universities as part of a 'general education' in the sense in which I have defined it.

Now we ought to question whether this state of affairs is, in fact, desirable. Should not science feature more largely in 'general education', and if it should, how can it be made to do so? It is my belief that it should. Those who seek a general education will often reach positions of importance in administration or management. They will, more and more as time goes on, have to deal with scientists and technologists and with matters which are scientific and technological. They must be ready to discuss and argue scientific matters with people who have been trained to think as scientists and they must be in sympathy with their points of view. Our

present arts graduates are remarkably good at this; they are, on the whole, a very able set of men and rapidly acquire some of the knowledge and mental outlook necessary for the purpose. But how much better they would be if they had started to make contact with these ideas and these people at the university and if they had made deeper and closer contacts.

I was interested to read, the other day, that of the 630 members of the present House of Commons, 335 had received a university education, but out of this 335, fewer than forty had any sort of scientific training or experience. That is an example of how little science contributes to the general education of those who govern.

On a smaller scale I came across an interesting example the other day of two cases where it might have been to some advantage if science had had some part in a general education. I met two recent arts graduates of my own college, who had gone down three or four years ago. One was a lawyer and the other a historian. In conversation I discovered that one was the secretary of a firm making digital computors, and the other was buying chemicals for I.C.I. Both were doing work for which their degree courses prepared them in only the most general sense. Would it not have been somewhat better if part of that general preparation had been through courses in science?

I suggest that it would, in fact, have been better, but that this science should not be provided through courses of the present type. I shall examine what sort of courses would be suitable and what sort of outlook they might lead to. I shall then suggest that the most valuable kind of general education at the university would be one in which science courses of this special type were combined with suitable courses in an arts subject. Neither alone would be so good as a combination of the two.

Before I start on the details I must make it clear that I am thinking in terms of an honours degree. The proposals are intended to provide for some of the most able students and there is no suggestion that they should lead to an ordinary degree, or to any soft option. They are intended for future leaders, and only the best is fit for them.

In asking what sort of science courses would be needed to provide a 'general education' in the sense I have described, we might start by rejecting what is of least value, from this point of view, in our present courses. For the man who intends never to practise science the scientific knowledge itself is of secondary value. The 'spectacular' side of science, the 'marvels of the universe' and the like, although those aspects of the subject are commonly dealt with in lectures and books designed for non-scientists, are, I think we would agree, not of great importance either. When topics of these two types are excluded the course would be found to be dealing more with the scientist than with scientific facts. It would be concerned with his methods of thought and his approach to his problems, with the way in which scientific advances are made, partly by flashes of genius and partly by the slow planned progress of alternating theory and experiment, and with how progress is often held up by some idea which is later proved to be wrong, or by the lack of some particular technique. It would show how different branches of science interact on one another, how ideas and techniques developed for one type of investigation are used in another, and how problems can sometimes be solved by a single worker with simple equipment, and at other times need teams of associated workers and elaborate apparatus. And as the account progressed it would become apparent that the intellectual pleasure derived from taking part in a scientific investigation, or even in studying it in detail, was largely an aesthetic one. It would be seen that many scientists are not the 'knob-twiddlers' of the popular concept, but are men engaged in intellectual pursuits of considerable complexity, whose delight is mainly in solving difficult problems, not in making complicated machines.

Now you will say that this is a somewhat vague idea of a course in 'Science as

part of a general education'. How can it be made more specific? I am qualified to make suggestions only about physics, and in that subject I think a suitable course would not be difficult to plan. It might, for example, be on the subject of radioactivity and nuclear energy. The essentials of the subject could be taught as a serious branch of physics, to any able undergraduate who had passed the G.C.E. at ordinary level in physics, chemistry and mathematics. The subject would be dealt with chronologically as it developed in practice. The early work of Rutherford and his collaborators provides an excellent example of inspired experimenting allied to brilliant thinking of an essentially simple nature; the later work introduces the need for large apparatus and teams of workers. The early theories were simple and descriptive; the later ones are complicated and mathematical if they are to be used by an original investigator, but they can nevertheless be explained simply if understanding is all that is required, and they lead to some consideration of the important philosophical ideas of relativity and indeterminancy. The subject leads naturally to a consideration of the limitations and possibilities of the use of nuclear energy.

Particular attention should be given to the part which would be played by practical work and mathematics in a course of this kind. I suggest that there should be many experiments designed to demonstrate phenomena, and that the students themselves should manipulate these when possible. Experiments, of the type common in standard physics courses, aimed at teaching techniques of measurement should not be included, but instead one or two practical problems should be set in which the student is invited to discover for himself what happens under certain conditions and to explain the happenings, if necessary with some help.

If the object is to understand the subject, rather than to advance it, it will not be found that much mathematics is necessary. What is provided should be kept very simple. Ideas of momentum, force, work, energy and potential would have to be taught but are not fundamentally difficult. One or two concepts would be introduced and elaborated for their own sake. For example, one might be the 'law of organic growth' leading to the ideas of exponential growth and decay, with reference not only to radioactivity but also to the growth of compound interest, of populations, of plants, and the like. Ideas of waves would be introduced physically and only the simplest mathematics would be used.

Students would be expected to read original papers, particularly those which describe the major advances, and those which summarise the state of the subject at a given time. Opportunity would be provided, in seminars, to discuss what made a clear scientific paper, and what an obscure one; which advances were important and which secondary; and why a discovery was made at a particular time.

There is no doubt in my mind that a course of this kind would be of serious intellectual content. It would, in fact, be at the level of part of the present courses for final honours in physics, but the ideas would be presented without any difficult mathematics. The whole concept of the course would be different from that of existing courses intended for students who are to become specialists, and it would need to be given by someone who was an enthusiast and believed in the educational objectives I have described.

It is suggested that a course of this kind would be of more value for 'general education' than a science course of the usual type largely because attention would be concentrated more on *thinking about* physics than on *doing* physics. Of course, for someone who is really going to use his physics, the doing of it, in the laboratory or by way of detailed mathematics, is of great educational value, but I doubt the value of that sort of thing for the person who is going to do something quite different when he leaves the university.

A person who had attended a course of this kind would have a fairly good knowledge of one important branch of physics, and of how it has developed. He

would have learnt, in considerable detail, how a completely objective argument can be built up on firmly established facts, and can lead to conclusions which can be tested by experiment. He would appreciate something of scientific method by seeing its application to one particular field of knowledge.

Now I am not suggesting that a course of this kind would by itself provide a good general education. It would need to be taken along with an arts course, and the two together would, I believe, be an improvement on the arts course alone. So let me now explain what I consider to be the deficiencies of the special kind of science course, taken by itself.

Scientific method is a very powerful tool, and one which a 'generally educated' man should certainly understand, but he must also understand that there are many problems with which it cannot deal. Some scientists tend to think that science has a monopoly of the processes of clear logical thinking from facts to conclusion, that this type of thinking is possible only in science, and that it constitutes scientific method. Of course that is not so. Any clear thinker uses the process of logic. What is peculiar to science is that it deals with that type of knowledge which can be made, as nearly as possible, objective or impersonal. It deals with facts which all right-thinking people would accept, and it leads to conclusions which they would agree upon. It insists on the testing of those conclusions by experiment whenever that is possible. It has been designed so as to exclude, as far as possible, the personal element. For these reasons most arguments in science lead to only one possible conclusion, which almost all scientists would accept. In science there is usually either no explanation of a set of phenomena, or one accepted one; it is unusual to find two or more explanations which seem equally plausible.

This type of objective, impersonal thinking, based on clear-cut established facts and leading to verifiable conclusions, is so important to the scientist that he often thinks it is the only valid form of thinking. It is essential that anyone who has to deal with scientists should know that, and should realise that any less exact form of thought may be considered suspect. To the future manager or administrator it is important to realise that there are large fields of human knowledge where this type of thinking can be done, and that there are a large number of scientists who believe it is not only the highest form of thought, but the only really satisfactory one.

But our future manager or administrator would be less effective than he might be if he, also, thought that this strict scientific thinking was the only reasonable one. He must understand it, but he must also know its limitations. He must know that there are wide fields of human endeavour where the premisses can *not* be so satisfactorily agreed and where the conclusions can *not* be so easily tested. A train of reasoning will depend on what the 'facts' are taken to be, 'judgment' will enter in an important way into the argument, and a conclusion may be reached which is radically different from that reached by other, equally able thinkers. Powers of expression and persuasion will then be of importance in getting others to decide which is the best solution. All this is foreign to the strict scientific method; it can best be learnt by studying an arts subject, and in my opinion that is why arts subjects provide such a good general education at present.

I am suggesting to you, however, that an even better 'general education' could be obtained at the university by spending half the time on a science course specially planned for the purpose and the other half on an arts course. Both should be at an honours standard. The science course should be of a special type not intended to teach science for its own sake, and should certainly not be exhaustive. I have outlined a possible one in a branch of physics, and I think this would be sufficient to inculcate the processes of thought appropriate to the physical sciences, including chemistry. A corresponding course might be possible in biology, based, say, on the ideas of evolution, and if there were time in one half of the three years at the

university, it might be desirable to have both the physics course and the biological one.

The other half of the time would be spent on an arts course. It is for others to suggest what might be possible. It seems to me that a good combination would be science and history, and my colleague Dr. Smail is to talk about that possibility later. Whatever the arts subject were, I think it would be well if some conscious effort were made, both by the science and arts lecturers, to bridge the gap between the two subjects. Obvious bridges exist in discussing history and philosophy of science, and both these subjects should play a part in the 'general education' I have in mind.

I must however make it quite clear that courses on history and philosophy of science would play only a secondary part in what I am advocating. My friend Stephen Toulmin has recently initiated a most interesting discussion of these courses, from which it has become clear that there is need for a new discipline in universities, dealing with history and philosophy of science and having its own staff and research school. I strongly support this idea, and in my own university, in Toulmin's, and in others we have gone some considerable way towards realising it. But my suggestions this morning are quite different. I want to see science taught by active workers in science, members of the science departments, to those who do not intend to practise science. I want to see it taught along with an arts subject, taught by scholars with reputations in their own subjects, and I want both the science and the arts lecturers to modify and adjust their courses for the purpose of providing 'general education' in the sense in which I have defined it. In addition I should like to see a little, but not too much, of history and philosophy of science taught, if possible, by those from the special school of history and philosophy of science. Then I think we shall be turning out from our university men who really are well equipped to undertake government, administration and management in a technological society, men—'of unmistakable intellectual breeding, whose trained judgment we can trust to choose the better and reject the worse'.

THE COMBINATION OF ARTS WITH SCIENCE COURSES IN HIGHER EDUCATION

By Dr. R. C. Smail, M.B.E.

A few weeks ago I assumed what many lawyers regard as the lowest form of life and became a juryman. Sitting in the quite surprisingly uncomfortable box, I listened to the examination and cross-examination of a series of witnesses, of whom the last was an expert witness. He was a scientist summoned from his laboratory to give his opinion that two lengths of metal had originally been all of a piece, and had been violently wrenched apart by human agency. He explained how he had examined the two jagged ends under a low-powered microscope, and had come to the conclusion that there was exact correspondence between the two sets of indentations. When taken with other evidence this looked bad for the accused, whose counsel exerted himself to throw doubt on the scientific witness. He suggested that the methods employed had been rather rough and ready. 'Could tests', as he put it, 'not have been made by spectroscopy?' The witness patiently explained that this was possible for organic, but not for inorganic matter. 'I must take your word for it,' said learned counsel, 'such terms are beyond me.' 'You are not alone in that, Mr. Smith,' came His Lordship's voice from the Bench; and, of course, there was laughter in court.

The implications of this incident are clear enough. Science is no part of the knowledge which an educated man need possess. Ignorance of its simplest terms are so culturally 'U' that it can safely be displayed in public with easy confidence.

The situation on which Aldous Huxley commented so brilliantly a generation ago still survives. There are still the two worlds of the cultural haves and have-nots; those on the one hand who are able to refer with easy familiarity to the great literary and artistic figures of the past—to claim kinship, as it were, with culture-uncle Virgil and culture-auntie Sappho—while outside this family circle are the rude mechanicals who have none of these things.

It is more than time that something was done to remedy this state of affairs. The central purpose of combining arts with science courses goes far beyond giving to arts men and to scientists a glimpse of each other's worlds. Its aim would be to produce men and women with a foot in each or, better still, educated people to whom there are not two worlds, but one.

It may be said at once that it is not inherently impossible for a student to take courses of both kinds. In the lists of candidates recommended each year for State Scholarships by the Schools Examining Boards there are always a few who have managed to combine, at Advanced level, an arts subject like English or Geography with their main papers in Mathematics and Science. At my own college a boy recently offered himself for admission who had taken Latin, Greek and Mathematics at Advanced level, and we were so astonished by this combination that, overfull though we already were, we found a place for him. But our astonishment was a measure of the rarity of the phenomenon. It is possible for sixth-formers to combine arts with science courses but, to say the least of it, such combination is not normal. Why?

Many of the reasons which lead children to specialise after they have taken O-level are plain enough. There is the force of tradition strongly established not only among many teachers but among many parents. There is the undoubted fact that many children reveal a specialising bent at an early age. There are the requirements both of university courses as they are at present constituted and of the open scholarships offered by the colleges of Oxford and Cambridge. The organisation of any school timetable is already a complicated matter, made still more complex by the limitation of accommodation and staff. Any additional combination of subjects can only aggravate the problem.

A word should be said about open scholarships, which are widely criticised as one of the strongest of the forces which impose over-early specialisation in the schools. Certainly they constitute a formidable obstacle to radical changes in the curriculum of Upper Forms. As is well known, these scholarships have long been offered in Classics, Mathematics, Natural Science, Modern Languages and History. More recently Geography and English Literature have been added to the list. A candidate offers only one of these subjects; and since some thousands of boys from a wide range of public and grammar schools compete for these awards each year, they do, indeed, constitute a strong encouragement to specialisation.

The situation, however, is not beyond remedy. For the past twelve years one group of Cambridge colleges have been offering scholarships not only in the traditional subjects, but they have offered as well a general scholarship. Candidates for this award are required to take an English essay and a general paper, and may offer three other subjects, to be selected from nine. It is a scholarship examination in which subjects must be combined, and in which arts may be combined with sciences.

Although there have been some outstanding exceptions, the general standard of the candidates who have taken this examination has not been as high as in the specialised subjects. Furthermore, the numbers have never been high either; and more than that, in recent years the numbers have so declined that the abandonment of the whole project may at least have to be considered. It would be a great pity if this attempt to meet the objection that open scholarships lead to over-specialisation were to die through lack of support.

From discussing these matters with my friends who teach in the schools, I know that there are many who would like to see more boys and girls continue both with arts and with mathematical and scientific subjects until a more advanced stage. The more enthusiastic among them believe that none of the problems of organisation is insuperable; they agree that the difficulties caused by open scholarships could be lightened by the addition of general scholarships; but nearly all of them hold the firm opinion that so long as most honours degree courses in the universities remain highly specialised, and until it is possible for undergraduates to combine the arts with the sciences in a main honours course, then one of the strongest incentives to changes in the schools will be lacking.

The two institutions are interdependent in these matters. The schools feel that it is difficult to change the organisation of studies in the upper forms without comparable changes in the universities, and certainly it is difficult for the universities to do so unless the schools keep them company. There are some arts courses, as there are some sciences, which can be started at the university practically from scratch; but the great majority of university courses begin where the school courses leave off. On the scientific side this is certainly true of a subject of central importance like chemistry; while the pursuit of the physical sciences in general demands a knowledge of mathematics of which the foundations can only be laid at school.

What, then, of this problem of introducing degree courses in the university in which arts and sciences are combined? Such a project immediately collides with that of training more scientists for technical work. From the point of view of those seeking to make good the shortage of scientists in industry, any plan which involves a student with a scientific training taking an arts course constitutes a serious diversion from a national need and must be discouraged. But it is an assumption of this paper that society has not one need but two; not only for practising scientists, but for men and women who will be administrators and executives and policy makers in whose education science will have played a part.

In some quarters a powerful source of university opposition springs from the high value which is rightly placed on specialisation—on the undoubted educational virtues of the intensive study in depth of a single subject, or a closely related group of subjects. In a university such specialisation is also imposed by the growth in the sheer volume of knowledge, which tends to press not only the graduate, but also the undergraduate student, into an ever-narrowing field. From this point of view, the satisfactory study of one single subject within the three years of an honours degree course is difficult enough of achievement; that of several subjects, much more so.

In commenting on this viewpoint it has to be said at once that there is no suggestion of attempting to replace the highly specialised courses of study which now exist. It is proposed only that there should be, in addition, courses in which arts and sciences are associated. There are already in certain British universities mixed courses of proved educational value in which different arts subjects are joined together. Why not arts with science?

Some advocates of specialisation are perhaps inclined to lose sight of one of the proper objectives of university courses. Any subject for undergraduate study can be used to educate in the sense in which Mr. Ratcliffe has used that word; it can also be pursued, technically and professionally, for its own sake. It has, it may be said, an educational aspect and a research aspect. Many discussions about programmes of study, examination methods and standards, reading lists and the like, come up against this dilemma: what is the ultimate object of the course: to produce an academic specialist, or a man whose future will lie outside the world of learning and teaching, and who will use the study of classics or history for the development of his faculties and his personality?

The two ends are not always mutually exclusive and both can often be served by the same measures; but the situation is constantly arising when one ideal or the other, consciously or unconsciously, explicitly or implicitly, must be chosen. And since those who make the choice are themselves not only teachers, but academic specialists increasingly pressed in the present climate of opinion to make research and the publication of research the first of their interests, then it is not surprising that the academic rather than the educational pole is often chosen. Students are directed towards the controversies and tentative conclusions of the latest research rather than to the treasures of past achievement, to current periodicals and journals rather than to the classics of a past generation. Science teachers, as Mr. Ratcliffe has told us, frankly set out to produce practising scientists who will continue to make use of their special knowledge. But there are many teachers of arts subjects who assume that their task, too, is to train researchers and professionals in their subject. Too heavy an emphasis on specialisation can lead them to allow wider educational objectives to fall into the background. From this point of view there is a great deal to be said for less rather than more specialisation in degree courses and for a wider variety of subject matter.

The proposed combination of arts with science courses at the university has also been found objectionable because of a supposed disparity between the methods and subject-matter of the arts on the one hand and the sciences on the other. It is argued that it is asking too much of any student that he should grapple with both at University level. But specialists are often inclined to exaggerate the 'special' characteristics of their own particular field of activity. They fall to the temptation of appearing before the rest of us as custodians of a mystery. In my own subject there have long been dramatic references to 'the historical method'. There are even professorial chairs with that title. Yet fundamentally what is the historical method more than method ? It is not only historians who must collect and assemble material, evaluate it accurately, interrelate it intelligently, generalise from it honestly. Scholars in all fields must proceed in this way.

If it is agreed that some of the main arguments against the combination of arts with science courses at the university are captious or misplaced, it remains to consider what kinds of courses may be possible. This is a major matter for the attention of educationists. In the few minutes remaining to me perhaps I may be forgiven if, as a teacher of history, I briefly discuss the possibility of combining my subject with scientific studies to provide a course for an honours degree.

This particular combination is well worth further exploration, if only for the reason that there is nothing forced or manufactured about it. Historians and scientists, each by their own route, have reached a stretch of common ground. There is no more important field of study for historians than that of the creative activities of the human mind, and in reconstructing the intellectual history of Europe scholars in recent years have been giving the scientific thinking and achievement of the past four centuries their due place in that history. Professor Butterfield has referred to the sixteenth and seventeenth centuries in words which have already been much quoted. 'The Scientific Revolution', he writes, 'outshines in importance everything since the rise of Christianity, and reduces the Renaissance and the Reformation to mere episodes, mere internal displacements, within the system of medieval Christianity.' Some scientists, for their part, have come to see, in Professor Toulmin's words, 'that they can acquire a more flexible and humane attitude towards their own specialities by learning to ask historical and logical questions about them'.

May I digress for a moment and take up a point made by Mr. Ratcliffe ? He drew attention to some educational differences between arts and science subjects, differences in the kinds of evidence used and in the possible generalisations, and in the degree of certainty of the conclusions which can be established.

Among the arts, historical studies certainly provide a strong contrast to some of the conditions with which science students are familiar. The historian's evidence is always incomplete, often fragmentary. It can usually be fitted together in more than one way to form more than one reasonable hypothesis, so that not only is technical skill required for the comparative evaluation of different pieces of evidence, but also the finest judgment in relating them to each other so as to build up the best possible interpretation of the whole.

The number of unresolved problems in history give it part of its educational value. The essays which are set to sixth-formers and to undergraduates are exercises not in the art of narrative, but in the art of reasoned exposition. In later life a student may well forget what he once knew of Charlemagne or Luther or the French Revolution; but among the things which he should have retained is the power to express himself with clarity, economy and eloquence; and by eloquence I do not mean verbal adornment or facility in making the worse cause appear the better, but the power of holding the attention of those whom he is addressing, so that they are not only able but willing to follow his marshalling of the facts and weighing of the evidence and to see the matter as he himself sees it. This power is needed not only by the politician and the advocate, the parson and the teacher. For better or for worse we live in a world of boards, syndicates, commissions and committees, and the more of us who can put our views to those around the table, either verbally or in writing, not only with precision, but with what I have called eloquence, the more smoothly and rapidly the day-to-day transaction of affairs in our society will go forward.

The organisation of the history of science has already made some progress in our universities. Departments have been formed, professorial, teaching and research appointments instituted, research is under way, and a number of courses are being given for undergraduates. It has to be remembered, however, that the purpose of some of these courses is different from that under discussion, which is not simply to make specialists in the arts or sciences more aware of what lies outside their libraries and laboratories, but to educate citizens, in a world in which an ever-increasing number of the problems are scientific problems, through the medium both of scientific and more traditional disciplines.

For this purpose the history of science, in *any* form, will not be enough. It may be, in the words of a recent *Times* leading article, that this new subject can provide in itself 'the basis of a liberal education'; but in certain forms it could fall far short of doing so. History is sometimes studied and written from the outside and results simply in the superficial description of the externals of the subject-matter. There are many histories of philosophy of this kind, which present to the reader a succession of thinkers together with a highly compressed résumé of the main themes in the thought of each. Such books are indispensable for a wide variety of purposes, but they do not lead a student to an understanding of the great issues of philosophy. They give him a chronological sequence and a table of contents; they enable him to follow change and to make comparisons; but they do not take him inside the subject-matter of the history. It is easy to imagine histories of science of this kind, even if they have not been written already, and it is history of this kind which, however useful as an introduction, would serve no deeper educational purpose.

It is to be hoped that educationists will give major attention to these problems. When they do, it is probable that they will be able to devise special courses to attain the ends which have been discussed this morning. But until they do the combination of history with science may be possible within the framework of existing courses. The syllabus of the honours School of History in many universities includes histories of various aspects of thought—a common example is political thought. Many of them, too, include a Special Subject, that is, the intensive study of a short historical period or of a limited and compact historical topic,

based on original sources, with the aim of knowing the subject-matter from the inside and of gaining first-hand knowledge of the problems involved in interpreting the evidence and of reconstructing its history. Within such a framework it should be possible to include a paper on the development of scientific thought, together with a Special Subject on a major scientific problem treated historically, in which an understanding of the science and of the history would go together. The candidate would need to be equipped at some stage with the necessary knowledge of mathematics and science, but this may have been done by his work at school, or, if not, it should be possible to provide him with instruction at the university in specially arranged classes, in the same way that some economists at Cambridge are taught mathematics, and some historians, economics. If this difficulty could be overcome, then the study of a scientific subject of a kind which Mr. Ratcliffe has outlined could perhaps be included, as a beginning, within an existing history course.

Both the last speaker in this session, and the headmaster of Dover College in a recent broadcast, have convincingly demonstrated a social need of growing urgency. The problems which now come the way of educated men and women engaged in the general affairs of government, administration and management are increasingly scientific, and our higher education needs to reflect that fact. As Mr. Peterson has said, 'our traditional humanist education should be enriched by taking on board more science'.

Some of the issues involved in bringing about such a change have been touched on in this paper, and if it leads to further thought and discussion on these problems then, however discursive, it will have been worth while.

SCIENCE AND INDUSTRY

The Supply of Scientists and Technologists

A Conference on *The Supply of Scientists and Technologists for Industry*, under the auspices of the British Association, will be held in the University of Leeds on July 5, 1957, commencing at 10 a.m.

The Conference has been arranged in connection with the Report of the Science and Industry Committee, published by the Oxford University Press on May 30, 1957, under the title of 'Industry and Technical Progress', by Professor C. F. Carter and Professor B. R. Williams, the directors of the two research units appointed by the Committee. (See page 494.)

The Conference will consider the problem of the supply of scientific personnel from the point of view of industry and also as it affects the schools, colleges and universities in this country. The Conference will be opened by Sir Henry Tizard, G.C.B., F.R.S., who will also sum up during the last session. Sir Ben Lockspeiser, K.C.B., F.R.S., will preside over the intervening sessions. The speakers will include Sir Alexander Fleck, K.B.E., F.R.S., Sir Christopher Hinton, F.R.S., Sir Reginald Verdon-Smith, Dr. A. D. I. Nicol, Mr. A. B. Clegg, Dr. P. F. R. Venables and Mr. C. H. Wilson.

The Conference is open to all who are interested in this subject. There is no Conference fee. Admission will be by ticket, and, as accommodation is limited, tickets will be issued in order of application, which should be addressed to: The Secretary, British Association, Burlington House, London, W.1—a stamped addressed envelope should be enclosed for the reply.

THE ECONOMIC EFFECTS OF AUTOMATION[1]

by

PROFESSOR H. D. DICKINSON

AUTOMATION may be defined as mechanisation plus automatic control. Thus, in a machine shop, automatic machinery may be already in use, but each job needs to be moved from one machine to another and put into position; the machine, even though fully automatic, needs to be started by an operator (even if it stops itself automatically), and various other operations require deliberate human control. In a fully automatic factory all these processes can be carried out by automatic control, which is thus a sort of super-mechanisation. Another example can be taken from chemical engineering, where long and complicated reactions are already automatised, the human agent being reduced to watching pointers on a control panel and making the necessary adjustments of flow-rates, temperatures, pressures, etc. The works become fully automated if these functions are taken over by automatic controls, whereby the pointers of the various meters and gauges themselves actuate the appropriate adjustments. It is not only in industrial processes that automatic control can be applied. Clerical work can now be done by electronic computers, making it possible for clerical staffs to be drastically reduced.

Automation is seen therefore to be something that is not totally new: it is simply a further development of a tendency that has long been with us. The economic problems of automation are essentially the same as those created in the past by the introduction of mechanised processes into industry, notably the displacement of labour by labour-saving machinery. However, it may well be that the magnitude of the problems likely to arise in the near future may be so great to as make the precedents of the past very fallible guides.

Little in the way of hard statistics is available; but individual cases of automation are on record where the *direct* displacement of labour is of the order of 75 or 80 per cent. (The net displacement of labour is always less than this because of the amount of *indirect* labour required to produce and service the automatic plant. Even so, the net displacement may be of a magnitude that recalls the early industrial age in Britain.)

As always with the introduction of improved mechanical processes, the saving of labour is not the only important aspect of the new techniques. One important result always is that, not only can old jobs be done with fewer workers, but new jobs can be undertaken that could never have been done at all with the old methods. A good example of this is the use of electronic computers, which not only greatly shorten the time taken to do ordinary calculations, but can perform calculations that would previously have required a quite impossible number of man-hours.

THE LONG-PERIOD CONSEQUENCES OF AUTOMATION

As in the case of ordinary mechanisation, the long-period results of automation may be expected to be almost wholly for the benefit of mankind. It will, in the long run, enable the joint efforts of human beings to produce more material goods with a less expenditure of time and effort than before, thus making possible a far higher degree of satisfaction of wants for all mankind. Of course, certain very long-range considerations obtrude themselves here: problems arising out of the use of leisure,

[1] Paper read before Section G of the British Association at Sheffield on Monday, September 3, 1956.

the sufficiency of supplies of natural raw materials, the very long-run consequences of population growth. These problems, I am confident, can be solved by a robust application of scientific technique and of a rational attitude towards problems of human ecology. It is to the problems of the medium-long period that I wish to direct attention.

These problems are of two kinds (interlocked, like all social problems), problems of labour transference and problems of income distribution. The first group of problems arises from the fact that any massive reduction in the labour-time taken to make a particular product nearly always makes redundant some of the labour at present employed in that branch of production, and consequently, if full advantage is to be taken of the saving of labour, this redundant labour must be applied to the production of other things that satisfy some human needs previously unsatisfied. To leave it permanently unemployed, or even to continue to employ it unnecessarily in its old job by some form of 'making work', is simply to frustrate the original labour-saving innovation. This means that we must look carefully at our social arrangements that are concerned with the flow of labour between one job and another.

The second group of problems arise out of the fact that big changes in the relative demand for labour and for capital, or in the relative demand for one kind of labour as compared with another kind of labour, tend to give rise to changes in the distribution of the social product as between labour and capital and as between workers doing one kind of job and workers doing another kind of job. If the average social product per head is increasing fast enough, it may be that everyone is so satisfied with the larger lump of cake that he has received that he will not bother to make comparisons of the relative sizes of his lump and of other people's lump. But, unless the average social product grows very rapidly indeed, it is probable that the absolute size of some people's lump of cake will shrink. Even if this does not happen, the tendency to look at relative shares and compare them in the light of some concept of social justice is so strong nowadays that I doubt whether even a rapid rate of general material progress will save us from serious social unrest and social conflict.

The Transference of Labour

(1) In the preliminary remarks I have said that labour-saving innovations nearly always make labour redundant in the employments where they are introduced. I said 'nearly always' because, in special cases, the demand for the goods in whose production labour is saved may increase by such an amount as will counteract the reduction in the quantity of labour required to produce each unit of the goods. For example, if the net saving of labour is 75 per cent and if the quantity of goods sold increases fivefold, the demand for labour will increase by 25 per cent; in this case, far from there being redundancy, the industry will attract labour from other employments. This may happen—it has happened—but it cannot be relied upon to happen, and in most cases it will not happen.

The conditions that are most favourable to such an expansion in demand are two:

(*a*) A general high rate of increase of the national income, of which the most likely cause is a rapid introduction of technological improvements in other branches of production.

(*b*) The willingness of producers in branches of production where technological progress is taking place to reduce the prices of their goods in proportion to the reduction of cost. Reduction of price tends to stimulate an increase in the demand for the particular good (independently of any expansion in the economy as a whole).

Even if this reduction of price corresponding to falling cost takes place, it by no

means follows that the resultant expansion in demand will reabsorb surplus labour in the automated industry. It may or it may not. How much the expansion of demand will counteract the initial reduction in the amount of labour required to produce each unit of output depends upon two factors: (i) the original proportion of wages to total cost; and (ii) the elasticity of demand for the product. If the original proportion of wages cost is high, and if the demand for the product is highly elastic, the increase in the number of units of the particular commodity sold may more than counterbalance the reduction in the amount of labour used per unit. In this case, the effect of labour-saving innovation will actually be to increase the demand for labour in the industry in which innovation is introduced. The technical triumphs of mechanisation in British industry that were achieved during the nineteenth century, and those that are being achieved in the United States at the present time, are due very largely to the existence of a competitive régime, in which entrepreneurs are forced to reduce their prices (at least after a brief delay) parallel with falling costs. In this way the benefits of mechanisation are spread as widely as possible throughout the entire economy, and promote an expansion of demand that absorbs the whole of the potential product of industry.

Unfortunately, the general trend of present-day economic organisation in Britain is away from this. Price reduction is considered almost indecent. Even those business-men who speak most loudly in favour of competition are apt to lower their voice when it comes to price-cutting. Trade associations whose purposes include the fixing of prices and the restriction of output are widely prevalent through British industry, although it is possible that the Restrictive Practices Act will curb their activities in this direction. Organised labour, faced with improved technique, has, as its first thought, the desire to stop its introduction, and, if this proves impossible, its second thought is to appropriate as large a proportion as possible of the benefit in the form of higher wages or shorter hours (or both) for the particular group of workers concerned. Neither organised capital nor organised labour seems ever to give a thought to the interests of consumers in general or to the efficient functioning of the economy as a whole.

(2) Apart from this special case of an automated industry actually expanding its labour force, we must consider the general case of labour-saving innovations. In general, we cannot look for such an expansion of demand for the product of the particular industry as will entirely offset the reduction in labour cost per unit of product. In most cases automation will result in the displacement of labour.

This general case is not, in the long run, a cause for disquiet. The labour that is redundant to one industry can be absorbed into employment by expansion in the output of all the other industries. General full employment can be achieved, even in the presence of a persistent tendency to redundancy in particular industries. Since the days of Keynes and Roosevelt, we know, in broad outline, how to do it. The measures required would be very largely (*a*) low interest rates (short-term), maintained by a vigorous monetary policy, (*b*) public works, (*c*) social services, and (*d*) redistributionary taxation designed to counteract any shift from wages to profits in the general distribution of income. It should be pointed out that, even with such measures, the more prices fall with reduced costs of production, the greater is the spontaneous tendency towards expansion in the demand for labour in the economy as a whole, and the less will be the need for vigorous measures *à la* Keynes. On the other hand, the stronger the tendencies towards the monopolisation, by either capital or labour, of the benefits of technological improvement, the more vigorous would be the counter-deflationary measures required, and the greater the social and political strains set up in a predominantly private enterprise economy (cf. the New Deal in the U.S.A.).

(3) Assuming that by appropriate measures we succeed in maintaining at a high level the aggregate demand for labour in the economy as a whole, what is the

general picture likely to be? The sort of reductions in the amount of labour used per unit of product that automation is expected to bring about makes it likely that any compensatory expansion in the demand for labour must be looked for in other industries and occupations. In other words, the introduction of automation will raise in a peculiarly acute form the problems of industrial transference and the need for a high degree of mobility of labour, both as between places and as between occupations. (Even if full compensation does take place in the automated industries and the demand for labour in them is not reduced, there will still be very big problems of retraining, redeployment and regrading, since, even if the same labour is employed as before, it will not be employed in the same way or on the same processes as before.)

This means that, if the full advantage of automation, in the form of a general increase in production and a rise in the real national income, is to be realised, it will be necessary to have a high degree of fluidity of labour, involving a willingness of workers to change jobs and workplaces and to accept the necessity of retraining for new jobs. It is obvious that emphasis on apprenticeship rules, on traditional craft demarcations, and on traditional rules regarding staffing of machines is entirely incompatible with such a state of affairs.

We have seen that the best chance of avoiding unemployment lies in the expansion of demand for commodities other than those affected by automation. What sort of commodities will in fact be demanded? The countervailing expansion in employment must be in occupations less suitable for automation; not necessarily in manufacturing industry, but also in transport, distribution, entertainment, and services. What are these occupations? They comprise most of those industries producing goods for highly individualistic consumers: most luxury trades come into this category, as do those producing for specialised groups of amateurs, such as fishermen, golfers, makers of model machinery, etc. Research, experimental work and most forms of repair and maintenance work come into this category. Unfortunately, with the tendency to mass markets, which automation will foster, and with the now widespread tendency to scrap early, instead of making do and mending, these industries, with one significant exception, are likely in the future to decline. This one significant exception is that of research and experimental work, the making of prototypes, etc.

Important fields of economic activity in which an expansion of the demand for labour is possible are those of transport, retail trade, and entertainment. In retail trade, however, and to some extent in transport, this development would run contrary to the present tendency to increased mechanisation even in these fields (examples being the one-man bus and the self-service shop). It remains to be seen how much personal service the shopper desires. Perhaps in the distributive trades a sharp division may grow up between the distribution of standardised goods in highly mechanised self-service centres on the one hand, and the catering for particularly discriminating and choosy customers, who are willing to pay prices that involve an extravagant use of distributive labour, on the other. (It may be noted that the practice of retail-price maintenance is not favourable to such a revolution in methods of distribution.)

Another important field for expansion is that of personal service. If the benefits of automation are monopolised by a small rich class, then, in the absence of sufficiently high taxation, the labour made surplus in industrial employment will be absorbed into the personal service of the rich. We may thus see a striking tendency of the last forty years moved suddenly into reverse. If the benefits of automation are more widely spread over the community, it is reasonable to suppose that there will be a big increase in services rendered to the community generally, such as those of teachers, doctors, dentists, nurses, etc. This may be possible under conditions of private enterprise; but it appears much more likely that the demand for

such services can be effectively organised only through an expansion of the public social services, financed out of taxation.

(4) But let us return to manufacturing industry, particularly to those branches which are undergoing automation. We have seen that, except under rather special conditions, if the full effect of labour-saving is to be achieved, the automated industry must lose labour to others. But what of the labour that is left in it? The effect of automation is to shift the demand for labour so that, instead of a large and variegated labour force, a small but very specialised labour force is required. Fewer —much fewer—unskilled and semi-skilled workers, perhaps rather fewer highly skilled craftsmen, but many more high-grade technicians will be required. Doubtless many of the less skilled workers will find employment in other industries. But, if automation becomes at all general, there will be a big qualitative change in the direction of the demand for labour. The demand for intelligent and highly trained technicians will be very great, relative to the existing supply. (In fact, there will probably be a famine of labour of this type.) The demand for ordinary labour will fall. If the demand for ordinary labour from other industries does not expand quickly (and, given monopolistic policies on the part of either capital or labour, there are reasons for supposing that it may not), the aggregate demand in the country as a whole may well fall below the supply. In other words, a considerable amount of general unemployment may easily coexist with a great shortage of highly specialised labour. This circumstance emphasises the importance of technical education and training. If automation is to come, we must get ready—now—to educate and train the specialised workers that it will need. Nay more, we must begin to educate and train those who will do the educating and training. The best preparation for automation is probably the expansion of our technical colleges and of their staff.

But, even if we do all that can be done in this direction, will it be possible to provide all the specialist labour that will be required? The answer to this question depends upon how educable the ordinary worker is, or, more precisely, what proportion of them are capable of being trained to do the new, specialised jobs. This is a field in which there is no knowledge, only opinion. In this country, with its reliance on traditional élites, both in the ruling class and in the ranks of skilled labour, there is a tendency to underestimate the possibilities of raising the general level of technical skill over a wide range of the population. In both the U.S.A. and the U.S.S.R., where the climate of opinion is more equalitarian, a much more optimistic view of these possibilities is taken, and a much greater effort is made to produce a technologically literate population.

Distributionary Effects of Automation

Automation involves changes in the relative intensity of demand for the various agents of production. As between capital and labour, it is likely to increase the demand for capital relatively to that of labour. As between the different grades of labour, it is likely to increase the demand, relatively, for the highest grades of skill and reduce the demand, relatively, for practically all other grades.

(1) *Capital and labour.* In a purely *laisser-faire* economy, one would expect that the result of a shift in relative demand away from labour and towards capital would be to reduce wages relative to property incomes. (This does not necessarily imply an *absolute* reduction of wages: if the average level of the national income rises high enough, a reduction of wages relatively to profits may still leave the absolute level of wages higher than it was before.) One can envisage a community in which the combination of rapid technological progress and monopolistic control of output would divert a very large proportion of the national income into the pockets of the share-holding class, and in which unemployment would be combated by the

development of habits of Babylonian luxury and profusion on the part of its recipients. Such an economy might be technically workable, but it would not commend itself to the moral judgment of the modern world. Any tendency of this kind would at once give rise to a politically irresistible demand for countervailing measures, such as more progressive taxation generally, the higher taxation of profits as such, and the nationalisation of industries in which automation was taking place or impending. If the State uses the proceeds of taxation in order to finance social services or income from civil rights (e.g. old-age pensions, family allowances, unemployment benefit), this will spread the benefit of innovation over a wider circle. If there is no spontaneous tendency to price reduction on the part of business firms, such a policy may be the next best way to spread the advantages of innovation over the entire economy and to promote a desirable expansion of demand.

(2) *Different kinds of labour.* It is almost certain that the forces of supply and demand will tend to raise the earnings of highly trained technicians and to depress, relatively at least, the wages of ordinary labour and also of the traditional crafts, which will no longer be in such strong demand. If, as a result of a rising real income all round (especially if general unemployment is successfully combated), the average level of real wages rises, the less skilled workers may not suffer an actual reduction in real wages but may only enjoy a less-than-average rise, while the earnings of the new technical élite soar to princely heights. But if our more pessimistic estimates of the possible expansion in the demand for labour are correct, the bulk of ordinary workers may face a reduced demand for their services, resulting in unemployment, or lower real wages, or both.

This brings up the political and social factor once more. With a trade-union movement like the British, whose numerical strength is now in the unskilled and semi-skilled echelons, and with a political labour movement that, in times of working-class prosperity, polls the votes of just under half the electorate, the probability is very high that the trade unions, and very nearly a numerical majority of the population, will react violently against any tendency to put into reverse the equalitarian tendency in the movement of earned incomes that has shown itself so strongly during the last fifteen years.

If labour is strongly organised in the automated industries, the workers (or some of them) may be able to appropriate for themselves a large part of the benefits of innovations. Two possible policies exist: they may demand the same wages and volume of employment as before, or they may demand the highest possible wages for a small number of workers.

(*a*) The first policy is that popular in British trade union circles: viz., no redundancy, all displaced labour to be reabsorbed *within the same firm.* (This illustrates the old quip that British trade unions will always agree to the introduction of labour-saving machinery provided they can make it quite certain that no labour is in fact ever saved.) This means that the labour cost is the same as before automation. The only possibility of cost reduction (and hence of price reduction) is if the new process is capital-saving as well as labour-saving. Except for this possibility the result will be the old cost, the old price, no expansion in demand, and no general progress. The only beneficiaries will be the people who happen to be employed in the automated factory when automation is introduced; they will receive the same pay for a fraction of the work.

(*b*) An alternative policy (more in line with American trade-unionism) would be not to oppose technological redundancy, but demand the highest obtainable wages for those workers who were retained. In this case, again, the wages bill would be kept at its old figure, with corresponding little chance for price-reduction and output expansion; but from the workers' point of view the result would be a small number enjoying extremely high wages and a much larger number thrown upon the labour market. If the rest of the economy was buoyant, with large investible funds

forthcoming to finance new industries, the displaced workers might be reabsorbed, but not at the high wages of the lucky few.

Both policies on the part of organised labour are likely to lead to glaring inequalities as between different bodies of workers in the same industry—inequalities of leisure in the one case, inequalities of pay in the other case. Even if an entire industry were automated, a wages policy of this kind would create great inequalities between workers in automated industries and workers in other industries, which had either not yet been automated, or where automation was in the nature of things impossible. (This last, as has been seen, is an important category.) This tendency to increased inequality between workers in different industries or between employees of different firms would be intensified by any general introduction of profit-sharing or co-partnership schemes, or even of a guaranteed annual wage on the model now popular in the U.S.A.

This aspect of the subject was brought home to me very clearly on seeing one of René Clair's films, *A nous la Liberté*. At the end of the film a gramophone factory has been completely automatised, and the factory owner, having got into difficulties with the police (for reasons quite unconnected with automation), disappeared, after handing over the title-deeds of the factory to the workers in it. The last sequence of the film showed the younger workers dancing happily in the open air while the older workers fished peacefully in the river. All the while there poured forth from the factory hundreds and hundreds of neatly packaged gramophones in the production of which no human being had done a single hand's turn. 'How delightful' was the spectators' immediate response. 'What about the workers in the *other* factories?' was the slightly delayed reaction of at least one spectator.

EFFECTS ON AGRICULTURE OF CHANGES IN THE BALANCE OF NATURE

SECTION M (Agriculture) devoted the morning of Friday, August 31, 1956, at the Sheffield Meeting of the British Association, to a discussion of the effects on agriculture of changes in the balance of nature. Mr. E. M. Nicholson gave an account of current ecological researches and what they mean to the farmer, both as regards animal populations and wild life management, direct and indirect. Dr. I. Thomas then presented a paper which dealt with the effects of trapping and shooting farm pests, and of natural occurrences on the populations of pests and their predators. The final contribution came from Mr. A. H. Strickland who showed that competition, climate and natural enemies control the steady densities at which insects are in balance with their environment, and that natural or induced changes in these factors materially affect the balance in the farmer's bank account.

Abridged versions of these three papers are printed below in the order in which they were delivered.

ECOLOGICAL RESEARCH AND FARMING

By E. M. Nicholson, C.B.

(*The Director General, The Nature Conservancy*)

May I begin by a definition of terms.

First, *ecological research* means studying the relations of plants and animals to their environment and to one another.

Second, *Nature conservation* means ensuring by ownership or control and by suitable management that the natural fauna and flora survive and flourish together with the rocks, landscape features, soils and water on which they depend.

Third, *Nature Reserves* are areas which serve either as outdoor living museums for nature conservation or as open-air laboratories for ecological research, or both.

Fourth, the *Nature Conservancy* is the official organisation (working under a Charter and Treasury grant like the Agricultural and Medical Research Councils) which carries out the statutory and other public responsibilities for both nature conservation and ecological research in Great Britain, under the supervision of the Committee of Privy Council for Nature Conservation which has the Lord President as its Chairman and includes the Agricultural Ministers.

The farmer, the forester and the nature conservationist are often thought of as being in fierce competition for land. That is only true of some rather small areas on the fringe of the three interests. All three are seeking to manage nature; the farmer for food and industrial crops, the forester for timber and the nature conservationist for scientific research and the preservation of outdoor living museums and for education, amenity, sport, and water conservation and so forth. Before we can manage nature for whatever purpose, we must understand it, and all three have a great common interest in the work which the Nature Conservancy is promoting for the fundamental understanding of how nature works, which we call ecological research. The problem now is to increase the flow of well-directed ecological research and to see that its results are as well understood and put into practice by farming and forestry as are the important contributions in the applied field stimulated by the Agricultural Research Council.

Nature consists of certain elements of fixed and movable capital and of dividends in the form of renewable resources. The fixed capital consists primarily of

the geology and the atmosphere which cannot be changed. The movable capital consists broadly of the topography which we can increasingly modify by bull-dozers, explosives and other drastic treatment; the soil which we can improve or deteriorate by our land management; and the water which we can either conserve or harness or dissipate or pollute or leave to its own devices. The dividend consists of the yield in water supply for all purposes, natural and economic, and the growth of vegetation and of animals including, of course, invertebrates. How to encourage and assist nature to build more capital is no less important a problem than how to exploit what nature happens to have formed and man has not yet dissipated.

It does not at all follow that research on fundamental processes of nature is less likely to yield practical results than research on the growth of particular crops or the diseases of livestock. Ecological research is not primarily designed to assist production, but is intended to assist the understanding, and the management wherever necessary, of natural processes. There cannot, therefore, be any reasonable conflict between farmers and nature conservationists over the need for ecological research.

Let me give a few illustrations of the ecological research now in progress and what it can mean for farming and forestry. First of all take soil research; Dr. J. D. Ovington of the Nature Conservancy's woodland soils and vegetation unit has carried out a detailed study of soil conditions in relation to a wide variety of species of trees. One offshoot of it is the experiment started during 1955 on the Forestry Commission's forest at Gisburn in Yorkshire.

These experiments show that in terms of dry matter the annual increment of an acre of land under certain conifers can be more than four times the yield of similar adjoining land under some agricultural crops. Moreover, the nitrogen and exchangeable phosphate of the soil may be higher after producing a crop of trees and lower after producing crops of, say, cereals for a similar period. On the other hand, Mr. Frank Law, Engineer to the Fylde Water Board, contends that the volume and value of the water supply sacrificed by afforestation on such a catchment is greater than can be justified by the value to the nation of the trees. It seems quite extraordinary that, although the facts are obtainable, this question should never have been asked, let alone answered, until now, in view of its great national importance.

Again it seems extraordinary that so little work should have been done on the extent and nature of soil erosion in this country. At Moor House, near the source of the Tees, the Nature Conservancy are carrying on a series of experiments on a 10,000-acre Nature Reserve, designed to test the effects of different types of treatment of the surface (including draining and burning) on runoff and also to test the possibilities of preventing peat erosion by encouraging the recolonisation of different types of vegetation. These experiments include high-level plantations of native trees, and if the trees continue to flourish as well as they have so far it will point the way to the provision of cover for sheep at high altitudes. It may also show how to arrest and reverse the process of soil deterioration which is occurring at a formidable rate on a great part of the hill lands of Highland Britain which constitute our greatest unused resource in land. The stumps of trees and the horns of cattle have been found by scientists at well over 2,000 ft. in places which will not carry either trees or cattle today. Even in living memory valuable pastures and sites of felled woodland have reverted to poor peat-moors.

Farming and forestry on the whole are living by exploiting the capital which nature originally produced by natural soil-building processes. Ecological research can show how these soil-building processes function and how to favour them. For example, recent research in North Wales has shown the existence of a distinctive race of wild white clover characterised by small size and extreme persistence and adaptability to the rigour of climatic conditions at high altitudes. The soil-building possibilities of this strain are now being followed up. Studies at the

Nature Conservancy's Merlewood Research Station in Lancashire point to the possibility that a better understanding of the role of different invertebrates in the soil litter system may open up fresh possibilities of improving fertility just as the discovery of deficiencies in trace elements have done.

The value of soil in economic terms is immense. It is surely worth spending something on finding out more about the obscure animals and plants which manufacture for us this great natural raw material. Many of the animals concerned exist in such quantities that on some fertile lands the total weight of the invertebrates under the surface exceeds the weight of the vertebrates grazing on it.

Other ecological research at present in progress is concerned with the effects of exposure to wind and the possibilities of wind-breaks, and with the study of evaporation and transpiration—one of the most important processes affecting the water balance and the growth of plants and animals. The results so far indicate that the evaporation rates can be extraordinarily high and differ considerably from those which had theoretically been predicted.

Extensive studies are also being made of the effect of grazing and browsing by rabbits, sheep and deer. Few people even yet appreciate how far the vegetation cover of a place is shaped by the teeth of grazing and browsing animals. The plants which are favoured by these animals are repressed and in some cases exterminated, while those which are avoided, such as bracken and ragwort, tend to spread and even to get entirely out of control.

This leads me to the question of animal population and its control.

Since this session is entitled 'Effects on Agriculture of Changes in the Balance of Nature' we may start by considering whether such a thing as the balance of nature really exists. It obviously does not exist in the old-fashioned sense that nature left alone will always tend to get back to the same patterns of distribution and numbers as existed before any interference took place. Nature has evolved over millions of years a host of plants and animals which are specially adapted to our climate and our soil conditions. Many of these native animals and plants which used to be widespread have been driven by land reclamation and other factors into very small bits and pieces of land such as undrained bogs or fens, woodlands, heaths and hedgerows. To the ecologist these wild places are the last reservoirs of the native stock of genes which, if exterminated, cannot be replaced in a million years. Who knows what further industries like the penicillin industry may spring from some obscure animal or plant which may not even yet have been identified, but may prove in a generation's time of priceless importance for human health or welfare? What seems to us a praiseworthy crusade to make the desert blossom as the rose, may appear to our descendants simply a rake's progress, comparable with the slum towns of the industrial revolution.

Apart from such irreversible changes, there is a tendency for each species to build up to somewhere about the same numbers and to occupy something like the same niche. Take for example, the common heron, a bird about which we happen to know a good deal, since the national census which I organised in 1928 has been repeated in 1954 and the intermediate fluctuations have been recorded by a team of observers annually. The results show that some mysterious force dictates with great precision and effectiveness that, however favourable the circumstances, there shall be less than 5,000 pairs breeding in the whole country and that particular colonies even shall keep within fairly close traditional limits.

It should be noted that a controlling factor certainly exists, and works with remarkable effectiveness and precision. It should also be noted that the heron is neither shot nor otherwise persecuted and that it has virtually no natural enemies; yet in fact, the heron holds its own numbers down without prompting or control. The idea that a species if not deliberately checked will continue to expand its numbers without limit is a complete fallacy. The first thing to find out about an

animal is what is the level at which the numbers will stabilise and whether this level is for some reason higher than we can accept.

A further point to note is that the controlling factors must lie in the direction of its food supply and the amount of suitable habitat. If we create new habitats, and new food supplies then we are making a vacuum into which some adaptable species is very liable to rush. For example, the wood-pigeon has few natural enemies in this country, the main one being the sparrow-hawk and to a less extent certain other birds of prey. It has been our national custom over several generations to slaughter as many as possible of these birds of prey. We have now added the national policy of planting large blocks of young conifers which make admirable nurseries for the nesting wood-pigeons, and have sown large acreages with field crops which are particularly attractive and helpful to wood-pigeons for rearing their young. A foreign ecologist would be astonished to learn that our aim is to reduce the wood-pigeon population to the lowest possible level, when it is quite obvious that ecologically what we are doing is to present the wood-pigeon with the most favourable conditions that it could desire for further increasing its numbers.

A weed has been defined as a plant growing in the wrong place and a pest might equally be described as an animal which does not know where to stop. What stands out clearly to the ecologist is that while man complains bitterly of pests man is himself one of the main agents in making pests of other animals.

For example, man brought rabbits to this island in the first place and kept them quite sensibly on islands and in specially contained rabbit warrens. For several generations they were kept reasonably in check by the high population of predators which then existed and by the rural population taking the surplus rabbits, either legally or illegally, but the greed for greater bags of rabbits helped the situation to get out of hand. With the agricultural depression of last century and the growth of commercial rabbit trapping the rabbit attained the dimensions of an overwhelming pest. The huge economic losses which were tolerated from rabbits before myxomatosis came to the rescue were a reflection of the confused and muddle-headed public attitude. The question now is, whether the altered atmosphere regarding the rabbit will lead to our keeping the rabbit population better under control in future, as we certainly can if we have the will to do it.

Let me sum up in this way. First, nature is not something separate from and in contrast to forestry and agriculture; it is part of the same continuous process. Second, it is vitally important to the farmer and the forester and to the nation at large that nature should be the subject of a properly directed programme of research and experiment. Third, this programme should be planned very broadly to take full account of the needs of agriculture and forestry for fundamental research as well as of the need for biological research and nature conservation. This requires a constructive and critical demand from foresters and farmers for the sort of knowledge which can be expected to emerge. There must also be an appreciation that the limited number of mainly small Nature Reserves should be regarded on the same footing as research laboratories which also may take good agricultural land but which yield a dividend of knowledge far exceeding the sacrifice involved. Fourth, it must be understood that it is an out-of-date approach to beat up nature with every conceivable modern machine and chemical and to seek to get the best results by short cuts. In farming and forestry as in nature conservation we must learn to work with rather than against nature, and we can only do this if we understand how nature works and if we adopt a somewhat more humble attitude. Let us, in particular, learn to show more understanding towards our fellow predators on this earth. Fifth, we should learn to regard ourselves as temporary occupants or tenants for life of a single large estate. The men who created and managed the great estates of England, Scotland and Wales appreciated that it is rarely a question o deciding whether this or that shall be done, but more often of deciding how bot

shall be dovetailed into a sensible and enduring pattern of land use and land management. Ecological research offers the possibility of developing an outlook and a tradition of land use and land management which could be as fertile and productive as that which they bequeathed to us, but which has now unfortunately lost its impetus. It can also reconcile the need for growing food and timber with the no less real need to keep alive non-material values on the land, and to keep the country worth living in for all its living creatures, including people.

THE NATURAL AND ARTIFICIAL CONTROL OF VERTEBRATE PESTS OF AGRICULTURE

By Dr. I. Thomas

(*Ministry of Agriculture, Fisheries and Food*)

From palaeolithic times man has left his imprint on nature—he himself is part of the natural scene, indeed, part of the animal kingdom; hence biologists regard the term 'balance of nature' with a certain amount of misgiving. Do we say that nature was balanced before man exerted his influence, or do we say that nature was balanced before, say, rabbits were introduced into Britain?

Many examples of man's interference with nature are quoted by Taylor (1955) in a talk on 'The adverse effects on animal life brought about by man's interference with nature'. It can be argued that the very appearance of the rabbit in this country can be put down to man. The extent to which man has changed the natural scene will be fully appreciated by anyone who has read *Dawn in Andromeda*, by E. C. Large.

In a book on the Balance of Nature, in 1909, George Abbey described vermin as 'wild animals of carnivorous habits, called ground [vermin] and wild birds of a rapacious nature, termed winged [vermin]' and many people would still define this dangerous word in the same way today. The dictionary says, 'mammals and birds injurious to game, crops, e.g. foxes, weasels, rats, mice, voles, owls, etc.' But throughout the length and breadth of the country there are differing shades of opinion as to which animals are beneficial, which harmful and which are vermin. If then we argue that land should be used solely for the production of food, both animal and vegetable, and that the maximum should be got out of every acre irrespective of housing, or factories, or defence, or amenities, or sport—then stoats and weasels immediately become beneficial. At present they are regarded as vermin because they kill ground game and winged game. Jays and magpies are vermin because they take the eggs of game birds, but stoats and weasels are among the chief predators of the rabbit, and stoats may take the eggs of the wood-pigeon. The wood-pigeon may also be kept in check by the jay and magpie, and even the grey squirrel may, on occasions, be beneficial in this respect. For the purpose of my talk, however, I have in some arbitrary way to distinguish between those animals which I believe to be harmful to agriculture and those which are beneficial, and I am going to deal specifically with the two most harmful, namely, the rabbit and the wood-pigeon.

The Rabbit

The rabbit itself is an introduced species—it is believed to have been introduced by the Normans (Thompson & Worden, 1956). Such an event as the introduction of an important new species into a community, is clearly a change in the balance of nature. What has this meant to agriculture? At first wild rabbits were kept in small communities in circumscribed warrens and they were not unreasonably regarded as a useful additional source of food because they did not take the place of animals which were more economic and better converters of herbage. Warrens were usually

constructed in coastal areas and along the boundaries of parishes where the activities of the rabbits would not impinge upon agriculture as then practised. In the Middle Ages rabbits were regarded as a delicacy and their value as food was very high.

In the United Kingdom a rough balance-sheet has been drawn up (Thompson & Worden, 1956). On the credit side the rabbit may be said to have been worth about £15 million per annum to the community; this includes the meat value, the fur value and the value of the finished products produced from the fur. The value of the raw material was only a fraction of the total and the income from rabbits actually received by agriculture was probably less than £2 million per annum.

In addition there are on the credit side certain intangible factors the value of which it is difficult to estimate—the pleasure afforded to children by the sight of live bunnies—the sport they afford to many a countryman, the training of gun-dogs and the additional protein which might not otherwise be consumed. Last but not least perhaps I should add that the shooting of rabbits is possibly the townsman's easiest method of satisfying his primeval hunting instinct. Put a townsman in the country and it is surprising how quickly he gets the habit of walking around with a gun—after all, most townsmen are only one or two generations removed from the country. The question is—how much are we as a country prepared to pay for these pleasures? In France members of La Chasse come from town and country and freedom to 'hunt' was one of the rights fought for in the Revolution.

On the debit side the damage to crops has been variously estimated at between £45 million and £60 million per annum. This includes damage to trees—the annual cost of protecting 750,000 acres of State-owned plantations is about £500,000. The damage to cereals carefully calculated as a result of statistically conducted experiments throughout the country was of the order of 5 per cent per annum of the cereal tonnage. The average yield of spring wheat, for instance, from unprotected plots was 15·2 cwt. per acre as compared with 18·2 cwt. per acre from protected plots. The damage to grassland can be estimated only from a few experiments. In west Wales, for instance, in one experiment the increase in weights of lambs on rabbit-damaged plots was 263 lb. compared with an increase of 521 lb. on rabbit-free plots. Similar figures were obtained at Wye where the increase of weight of sheep on rabbit-grazed plots was 64 per cent less than on rabbit-free plots. The total loss of herbage from rabbits is thought to equal the losses to cereals.

To these figures must be added a considerable sum necessary for the repair of ditches and banks, the depreciation of the value of land and the possible role of rabbits in the spread of certain important diseases of domestic animals.

How did this situation arise?

Rabbits eventually spread from the warrens into cultivated and well-cared-for land. At first, however, they were kept in check because the land was mostly in the hands of small or large estate owners and it was usually well keepered. Rabbits were snared, ferreted and netted; hares were usually present in some numbers and winged game was well looked after. Subsequently, when rail transport became cheap and the demand for rabbit skins and carcases increased, the trapping of rabbits became a remunerative occupation and particularly during the First World War open trapping with the steel gin trap led to much increased catches. When such trapping started on farms in Carmarthenshire, for instance, foxes, weasels, stoats, cats and dogs were usually caught in some numbers, in addition to rabbits. (Buckley (in Kirkman, 1934) has, for instance, cited the case of a farm in Pembrokeshire of about 250 acres, where on the first trapping only 250 rabbits were caught, but there were in addition twelve foxes, between thirty and forty weasels and stoats as well as cats and dogs. In subsequent years, 5,000 rabbits were caught annually on this farm, the hedges and fences became dilapidated and the farm became more or less

a ranch. Where previously the rent of the farm was £1 an acre, following the increase of rabbits it could not be let at 5*s*. an acre. On another farm of about eight acres, in the first round the trapper caught twenty-one cats. During the First World War trapping thus became an industry in west Wales to the detriment of farming and other field sports in the area. This has been attributed by Buckley to the use of the steel gin trap and striking confirmation of this view has been obtained in a few preliminary experiments we have done on the open trapping problem. In one experiment, using a gin and a humane trap in the open, we caught thirty-seven rabbits; but in addition to a few birds we also caught three grey squirrels, three cats and one pole-cat. There was in addition evidence that the traps had caught but failed to hold two foxes, one badger and a few domestic animals, to say nothing of one farmer!

It is possible that the killing of a comparatively small number of predators would allow the rabbit population to build up to numbers which would become out of hand; the result would soon be the disastrous condition known as rabbit-farming. Quite apart from this aspect of trapping, other considerations tend to favour the build-up of the rabbit population. For instance, trapping is done only to take off the 'cream' of the 'crop' with sufficient residue being left for breeding up quickly in subsequent years and, what is more, it has been shown that by intensive snaring following gin trapping in the open, many more does are caught than bucks. In areas where rabbit farming was practised, individual farmers attempted to stand out against these measures, but usually they had to succumb to pressure from all around them because of the very large numbers of rabbits moving in from neighbouring farms. Matthews (1952) in his book on British Mammals also makes the point that the great increase in the trapping of rabbits has been responsible for the decrease in predators. Of stoats, he states that in south-west Wales, they have been locally, but probably only temporarily, completely exterminated. The destruction of carnivores, he says, 'dates from about the middle of the last century or a little before, and its onset coincides with the introduction of the steel trap or gin. Whatever may be said about trapping animals, it cannot be denied that this is a cruel device that has been responsible for the death of great numbers of the predators as well as of the prey.'

It is difficult to tell whether myxomatosis comes under the heading of natural or artificial control. It was anticipated that as a result of the calamitous decline in the rabbit population due to myxomatosis, foxes would become hungry and would attack domestic animals on an increasing scale. It is known, for instance, that over a period of years the numbers of foxes killed in Wales by Fox Destruction Societies may be measured in thousands. The figures are: 1949, 7,739; 1950, 8,321; 1951, 7,072; 1952, 6,463; 1953, 5,105; 1954, 6,307; 1955, 8,004.

It was also known from some work done by Southern & Watson (1941) that in an area where rabbits were numerous, some 50 per cent of the diet of foxes consisted of rabbits. One would have thought then that if rabbits were taken away foxes would be adversely affected. It was natural, therefore, to step up work on the control of the fox, and this was done. In 1955, 8,004 foxes were destroyed by Fox Destruction Societies in Wales, but this figure is lower than 8,321 foxes destroyed in 1950. I think the reason why we have not had large numbers of hungry foxes and serious depredations on domestic animals is the fact that the fox is a most omnivorous animal and I think when rabbits are available it will feed on rabbits, when rabbits are not available it can still fend for itself. For instance, in some preliminary work we have done on the fox diet, there had been a considerable increase in the numbers of the short-tailed vole and the brown rat eaten, and the amount of vegetable matter had also increased. Indeed some vegetable matter was found in 57 per cent of the stomachs examined.

The effect of increased growth of wild herbage has been studied in detail by

Dr. A. S. Thomas of the Nature Conservancy. But an incidental effect of this increased herbage will probably be a large increase in the population of voles (*Microtus* species). So far this does not seem to have occurred, but I think it is very likely that if the rabbit population is maintained say at about a quarter of what it was before, we shall then have periodic increases of the vole population with a flare-up of damage to young forest trees.

This history of virus diseases such as myxomatosis generally follows a set pattern. The virus itself becomes attenuated, and the host builds up its resistance. We all imagine that this will also happen in the case of myxomatosis and there is evidence that it is taking place in Australia.

What are the remaining factors that tend to check the rabbit population? A natural population regulation factor of some importance was discovered by Brambell & Mills (1947, 1948), who found in Caernarvonshire that in the early part of the season some 60 per cent of all the litters conceived were resorbed. But it is likely that this factor becomes economically significant only when the numbers of rabbits are quite beyond those which can be tolerated by good farming.

Of the diseases of the rabbit liver-fluke is probably one of the most important, not altogether because of its effect on the rabbit but because it parasitises domestic animals, including sheep and cattle. Only occasionally does the rabbit population itself suffer and then only in wet habitats where the intermediate host, the mud snail *Limnaea truncatula*, is able to exist in sufficient numbers to be a reservoir. Both nematodes and cestodes also occur in the rabbit and, here again, these parasites are of significance because the rabbit is an alternative host. This is especially true of the larval stages of *Taenia pisiformis* and *T. serialis*.

Other diseases just worthy of mention are coccidiosis, which probably exercises some degree of control, necrosis of the liver, rabbit syphilis, toxoplasmosis and tularaemia.

There remains for me to mention a few artificial methods of control. Rabbit-proof fences can be of value only in confining rabbits to small limited areas or keeping them out of small areas—on a large scale they break down because of the impossibility of supervising adequately every yard of fence as frequently as necessary. This has been proved up to the hilt in Australia where the enormous barrier fences failed to check the advance of the rabbit.

Then there is shooting and ferreting, which are both useful adjuncts to other methods, and repellents, which at best are palliatives. Gassing is perhaps the most efficient method for use in Great Britain but it needs to be done with great care and thoroughness. Best of all is good farming and avoidance of the temptation to combine farming with game preservation to the extent that the latter becomes as important as farming itself.

The Wood-pigeon

Where rabbits have been seriously reduced, wood-pigeons are now perhaps the farmers' greatest vertebrate pest. But as with rabbits it is probably true to say that man has created just those conditions which have allowed pigeons to flourish and multiply. In the course of hundreds of years and intense cultivation, man has not only changed the character of the countryside but in some instances he has produced in profusion the food required by certain animal and bird species. In the case of the pigeon he has recently increased the breeding habitat preferred by the species. Forest and woodlands have been planted on a large scale and although the pigeon does not favour building nests deep in forests, it relishes the conditions produced along the periphery. Man has planted—and this within comparatively recent years—enormous acreages of the foodstuffs preferred by pigeons, that is, brassicae and—perhaps in a more restricted area—peas. In the Eastern Counties peas have become a most important and lucrative crop for the farmer. The gross

returns from some pea crops may be as much as £140 per acre. A farmer is therefore prepared to pay quite large sums of money to ensure that such a crop is harvested and naturally he brings all the pressure he can to bear on the scientist who is working on the control of such a pest. The farmer is usually prepared to co-operate fully and enthusiastically with the research worker, but he is usually a bit worried about what his neighbours will think if there is only just a small possibility of game birds being accidentally killed in the process of trying to find measures for combating the pigeon pest.

The annual mortality of adults of most birds is very high and varies from 30 to 60 per cent, and that of eggs and young is higher, being in the region of 70 per cent in the wood-pigeon. Any control measure, therefore, which has as its aim the killing of eggs or young, is likely to leave little impression unless it is very efficient and, to a lesser extent, the same applies to the killing of adults. For instance, in the year ending December 31, 1955, over 2½ million wood-pigeons were said to be killed by shooting and in the first quarter of 1956 it is estimated that as many as 445,000 pigeons were killed in England and Wales. Admittedly these are estimates, but even if half this number were killed, it is, indeed, very large. In this quarter the expenditure on cartridges alone was probably over £28,000 and the subsidy on these is about 50 per cent. In a few years, therefore, it is easy to see that wood-pigeons have cost the country large sums of money. It is not yet possible to assess the damage with accuracy but sampling experiments are now being made.

Other methods of dealing with the pigeon are by scaring and by the use of narcotics; nearly all birds learn so quickly that any method of scaring has to be very frequently changed in order to stand any chance of being successful even very locally for short periods. As to the use of narcotics I can only say that we are at the stage where we know we will have to learn far more about the habits of the pigeon before this method is likely to be successful.

Of the diseases of pigeons the most common are pseudotuberculosis, caused by a *Pasteurella*, pigeon pox, coccidiosis and avian tuberculosis (McDiarmid, 1948). Although locally a number of pigeons may be killed by one or more of these causes, I do not think any of them is likely to become an important factor in the control of the species.

Here then the choice before us is: (1) to revert to the old balance whereby there were fewer breeding sites; (2) to disperse far more widely the crops which are attractive to the species; (3) to control the bird by artificial means. I suppose this is a case of Hobson's choice, but it is certain that any artificial means that are devised will have to be drastic and large numbers of birds will have to be killed if farmers are to see the benefit in reduced losses to crops. With insects, for instance, it is possible to control a pest on one particular field. Even with rabbits it is possible to control a pest on one particular estate, but with pigeons the whole of the pigeon population of the country probably has to be affected before a beneficial result is obtained.

The theme of my talk seems to have been that man has himself induced, or at least created, the conditions for changes in the balance of nature. As Taylor put it, 'There can be no doubt that the arrival of man was one of the worst calamities that ever happened to all the creatures of this lovely earth'. It is, therefore, up to man to look ahead and to foresee and avoid the consequences of some of his actions instead of wasting his substance in vain attempts to remedy dangerous situations of his own creating.

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INSECT PEST BALANCE IN AGRICULTURE

By A. H. Strickland

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For the past thirty years animal ecologists have been trying to verify experimentally two mathematical theories. The first of these, the Logistic Curve, is commonly known as the Lotka/Volterra theory (Lotka, 1925; Volterra, 1931), and is concerned with the growth and decline of populations of a single species. The second, or Nicholson/Bailey theory (Nicholson & Bailey, 1935) is concerned with the growth of mixed populations of a parasite and its host.

In both theories the development of a population is, in a sense, predetermined by basic postulates, and biologists are far from agreed on the validity of these. In particular rather specialised and unusual usage has been made of words such as 'competition', 'control', and 'balance'.

In the strict, physical, sense, for instance, there is no evidence that 'balance' exists in the artificial conditions imposed by laboratory experiments with insects over a period of forty generations (Park, 1948; 1955), and even less that it exists in nature.

On the other hand we all know of hypothetical examples such as Herrick's (1926) estimate that if all the progeny of a single female cabbage aphid lived, they would weigh over 800 million tons after a $4\frac{1}{2}$-month period. Observation and experiment have shown us that this situation is never likely to arise in nature, because populations of any given species are restrained from unlimited increase by the action of one or more components of their environment—and here I include in the term 'environment' everything external to the individual members of the population.

The precise factors operating in any specific case can only be determined by prolonged and careful study. After a long series of field experiments, as well as detailed laboratory work, for example, Andrewartha & Birch (1954), who are proponents of the probabilistic school in ecology, suggest that plagues of the grasshopper *Austroicetes cruciata* will occur in certain favourable areas five or six times in a hundred years, in less favourable areas three times in a hundred years, and in areas at the edge of its present distribution about three times in a thousand years. But it is impossible to forecast when, within a hundred-year period, a plague is about to start, or when it will decline. In a similarly probabilistic approach, Park (1955) concluded from a long series of replicated laboratory experiments on two species of flour beetles living in competition that: '. . . the dependence of the survival of a species on the characteristics of the environment appears to have the nature of a chance event (not necessarily a 50–50 event)'.

It seems impossible to go beyond this stage at present: a more precise appreciation of the problem of insect abundance will only come through greatly increased study combined with the use of stochastic models of the type recently described by Skellam (1955).

From an agricultural point of view we are interested in the abundance of pests in relation to their distribution throughout the country, and to the amount of economic damage they do. In these circumstances intensive work on one or two field sites is quite inadequate. For example, Barnes' work (1956) on the wheat blossom midges (*Contarinia tritici* and *Sitodiplosis mosellana*) refers only to the rather special conditions prevailing on Broadbalk field at Rothamsted over the years 1927 to 1955. Work on pest populations and the environmental factors which influence them must be carried out concurrently on an appropriate number of field sites if conclusions of general validity are to be reached.

Long-term work on pest populations, especially if adequately replicated, is expensive and before a specific investigation is put in hand it is necessary to demonstrate unequivocally that the expenditure is justified. In some cases, mostly in continental areas, pests are so serious that funds are forthcoming almost automatically. Nobody, for example, needs convincing of the immense damage done by locusts in Africa and the Middle East; it is the gradual reduction in yield caused by small numbers of pests which are active over an extended part of the growing season, which are difficult to demonstrate, but which, on a long-term basis, are of importance, that need emphasis here.

It was with these facts in mind that, ten years ago, the Conference of Advisory Entomologists started the routine assessment of the annual peak population densities of about a dozen insect pests throughout England and Wales, and six years ago the scheme was widened in scope to include detailed work on population density in relation to economic damage. Since already the results are of significance I should like to describe two of the projects in some detail.

Cabbage aphid (*Brevicoryne brassicae*) was chosen for detailed treatment because it is a stable species which is restricted to cruciferous hosts throughout the year. It seemed possible that conclusions reached from a study of this quite simple species might be applied to other taxonomically and biologically more complex aphids.

An 'Extensive Survey' was started in 1946 which involved the assessment of peak aphid density on a substantially random sample of about one hundred unsprayed Brussels sprout crops throughout the country. This has been carried out every year since.

The second phase of the work started in 1951, when field plot experiments were set up in commercial sprout fields in the main growing areas. The trials were designed so that the aphids behaved naturally on some plots but were prevented as far as possible from colonising the plants on other plots. Reasonably accurate estimates of aphid density were made on all of the aphid, and essentially aphid-free, plots at each site at regular intervals from June until October, and finally the plots were harvested individually by skilled commercial pickers who were told only to take those sprouts which, from experience, they knew to be marketable under current conditions. The work was repeated for the next four years, at the end of which we had results for twenty-two 'site-years' covering a wide range of agricultural and meteorological conditions.

We now know that cabbage aphid damage to Brussels sprouts alone, expressed as acreage equivalents, amounts to 7,500 $\pm$ 2,400 acres every year. In monetary terms, in nineteen years out of twenty cabbage aphid will involve the sprout-grower in losses of anything from half a million to $1\frac{1}{4}$ million pounds, the average over the past ten years being £868,000 per annum. What is perhaps of greater interest is that if all the sprout fields which justified treatment, in relation to the current costs of such treatment, had been sprayed with an organo-phosphorus insecticide at the normal time—that is, in August or early September—aphid damage would still have cost about £500,000 per annum due to the quite small numbers which were present on the plants from their emergence in the seed-bed

onwards. Another interesting by-product of this work is that the Advisory Entomologists can now tell farmers, with some confidence, whether there are sufficient aphids on their sprout crops at a given time to repay the cost of spraying.

Work which is perhaps of greater general interest was put in hand on frit fly (*Oscinella frit*) of oats in 1946, and was followed by field plot work which was started in 1952 in collaboration with W. F. Jepson, of the Imperial College Field Station, Silwood Park. As with cabbage aphid, we now have ten years' extensive survey data relating to grain infestation at harvest. This work has been rather more detailed than that on the cabbage aphid. With frit fly we attempted four treatments: complete control, control of the first generation which attacks the seedling crop only, control of the second generation which attacks the emerging ears at panicle burst only, and no control at all. Although, pending analysis of the complete five years' data, we cannot attach statistical probabilities to our conclusions, some of the results already seem sufficiently clear cut to justify comment. First, on average in England and Wales, about 3 per cent of oat shoots are killed at an early stage by first generation larvae, and about 5 per cent of oat grains are damaged by larvae of the second generation. At this average level of attack the first generation reduces yield by approximately 1·8 cwt. per acre, while the second generation causes a further loss of useful grain of the order of 3·5 cwt. per acre. I must emphasise here that our estimates of shoot attack refer only to the position during the month of May. A visually assessed 3 per cent attack early in May almost certainly implies a rather greater loss of earing shoots over the season as a whole.

In all, present indications are that frit fly costs the farmers in England and Wales approximately £10 million per annum—the national oat crop over the past ten years has been worth roughly £34 million per annum to the farmer. But for frit, the same acreage and effort would have produced a crop worth £44 million.

Here we have a hidden loss which clearly warrants careful investigation. At present we can say little more than that complete frit control may well increase the national average oat yield from 18·6 to about 24 cwt. per acre.

There are two sure ways of reducing pest density: to kill the insects outright with natural enemies or chemicals, and to reduce in some way or other their innate capacity for increase. Because of the relative ease with which toxic chemicals can be applied, and their rapid action, they are widely used nowadays. It is obvious that this state of affairs would not have arisen if considerable benefits did not accrue from their use. But they are at best palliatives, and are unlikely to provide a permanent solution to the problem. Their indiscriminate use on a large scale is, in fact, very likely to result in the segregation of strains of insects which are resistant to them, or outbreaks of quite new pests previously held in check by natural enemies, as Ripper (1956) has pointed out in a recent review. So, except in the sense that elimination of the last local population means the extinction of the species, it is unlikely that pests will be eliminated by chemical means.

The only way in which pests can be permanently reduced is by so affecting their environment that their 'actual rate of increase' is reduced. This implies changes in agricultural practice, either in the crop varieties grown, or in cultural operations. Milne (1952), for instance, has shown very convincingly that the sheep tick (*Ixodes ricinus*), which is very troublesome in the northern counties, can be substantially controlled by minor alterations in flock management coupled with a degree of re-seeding of old moorland pastures, while it is well known that the rate at which wheat bulb fly (*Hylemyia coarctata*) density increases can be changed by suitable rotational cultural practices (Raw, 1955).

In practice farmers are often reluctant to adopt cultural methods of pest control because they cannot accurately assess the benefits in relation to the cost. This is an aspect of applied ecology that is emerging from the co-operative survey and field plot work which the Advisory Entomologists are carrying out. We can now say

with some confidence that cabbage aphid infestations will be less severe on fields of five acres than on large fields, even in areas where brassicas form the major cash crop. The possible advantage, at the grower's average price over the past ten years, is about 50*s*. per acre. Few large sprout-growers would consider changing their established system for a return of this size, though some might be tempted to break up large fields of sprouts by drilling a few rows of a cereal at appropriate intervals. On the other hand an advantage of about £8 per acre will on average be obtained by the man who sets his sprouts in fields which are situated at an altitude of more than 100 ft. above sea-level and well away from other cultivated brassica crops. Again there are disadvantages in the proposal, but at least they can be assessed in financial terms, and in the present case it is quite clear that the host crop can be, and is, grown commercially in conditions which are below the optimum for the aphid.

There is one further method by which the actual rate of increase of a pest can probably be greatly reduced. A good example is provided by the work on the eradication of screw worm (*Callitroga hominivorax*) in the Dutch West Indies and parts of the United States through the use of male flies that have been sterilised with gamma radiation. The technique is difficult, and its application limited and costly. For instance, twice as many sterile male flies have to be released as there are normal males in the area to be treated. In the island of Curaçao, which has an area of about 170 square miles, 16 million fly pupae were treated with gamma rays and subsequently released to ensure that half this number of sterile males entered the endemic breeding areas. The sterilised males, of course, mate with normal females who then lay sterile eggs. In a full report which has recently been published Knipling (1955) claims that screw worm has apparently been exterminated in Curaçao by this method. Here it is interesting to recall that the U.S. Atomic Energy Commission has noted marked ecological changes over a three-year period in the waters of the White Oak lake in Tennessee. These waters are only very slightly polluted with atomic waste, yet the rates of reproduction and growth, and the longevity, of fishes is being diminished, and two species have already practically disappeared (Fontaine, 1956).

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ANTARCTIC RESEARCH

SECTIONS C (Geology) and E (Geography) devoted the morning of Tuesday, September 4, 1956, at the Sheffield meeting of the British Association to a joint discussion on 'Antarctic Research'. Sir Raymond Priestley, M.C., President of the Association, opened the symposium from the Chair with some introductory remarks which are printed below. Dr. V. E. Fuchs, leader of the 1955–58 Trans-Antarctic Expedition, then gave a paper on the plans for the expedition, which included a description of the establishment of Shackleton Base, and referred to the geography of the area, and to the proposed geological and geographical work. For obvious reasons it has not proved possible to secure a text of Dr. Fuchs' paper, but reference has been made to it in *Nature*, **178**, October 13, 1956, pages 773–5. After Dr. Fuchs had spoken, Dr. R. J. Adie, of the Department of Geology, University of Birmingham, presented a paper on 'Geological Research in Graham Land'. He was followed by Mr. D. H. Maling of the Department of Geography, University College, Swansea, whose contribution was entitled 'Some Geographical Results of the work of the Falkland Islands Dependencies Survey, 1944–55'. Mr. H. H. Lamb, head of the World Climatology Branch of the Meteorological Office, then spoke on 'Winds and Weather in the Antarctic'. The last invited contribution came from Dr. W. Campbell Smith, C.B.E., M.C., lately Deputy Chief Scientific Officer at the Natural History Museum. The title of his paper was 'Some Geological Results of the Expeditions to South Victoria Land up to 1914'.

Abbreviated versions of these papers are printed below in the order in which they were delivered.

INTRODUCTION

By Sir Raymond Priestley, M.C.

The great difficulty of a Chairman who wishes to make a contribution to a symposium like this is the fear lest he should extract some of the plums from the puddings of his successors. I just want to emphasise three points about Antarctic research—and particularly Antarctic geological research—from my own experience in the field.

The first is the great good fortune that attended the work of the expeditions of the first fourteen years of the present century up to the First World War. When the war put an end to this first twentieth-century wave, Antarctic palaeontology, and, more significantly, Antarctic palaeoclimatology, was known in its broad outlines much as it is seen today. We knew from East Antarctica of Cambrian Archaeocyathinae; Devonian fish; Permo-Carboniferous coal measures with the Glossopteris flora, and Rhaetic trees. Evidence of prolonged and extreme cold only dated, at the earliest, from late Tertiary times. In West Antarctica, beyond the Ross-Weddell Graben, the Swedes had found Jurassic, Cretaceous and Tertiary floras proper to a quite different climate from that which prevails today. Not until the late Tertiary, again, did fossil penguins of large size, and other evidence, suggest the onset of severe cold that was to culminate in ice-sheets that we know were in places a thousand or two feet thicker than the present Continental-Ice and that extended, in places at any rate, for many miles beyond the northern limits of the present coastal land-ice cliffs.

So far, until 1956, nothing of great economic value has turned up. But I think the significance of what happened in those first few years is this. Another stroke of luck such as that enjoyed by the early explorers might similarly open up economic possibilities of which we dare not at present even dream.

I personally think the most likely place for such discoveries might be the new New Zealand McMurdo Sound base, situated as this will be at the foot of hills formed from the Pre-Cambrian basal complex, intruded with granite bathyliths, which forms the peneplain upon which the horizontal Beacon Sandstone series rests. We know that hereabouts the complex is shattered by faults, criss-crossed with dykes and sills of quartz dolerite, and broken through by older trachytic and more recent alkaline lavas of peculiarly intriguing type.

Discoveries of mineralogical interest, perhaps even of economic importance, may equally well be made—given similar good fortune—by sledge parties or helicopter flights south-west along the Antarctandes from the southernmost bases of the Falkland Islands Dependencies Survey. I say no more than that.

But this problem of the intervention of chance in Antarctic geological research brings me to my second point, which is the very considerable handicaps under which field geologists in the Antarctic work. To begin with there are months on end when in latitudes in the seventies and higher the cold confines them near to base and the dark restricts their powers of observation even there. In summer travel the geologist has in any case only the fag-end of the day in which to work. In addition, periods of 'white-out' as our American colleagues, with their flair for enlarging the English language, have come to term those far too common days when glare eliminates both horizon and shadow and snow-blindness is a menace, compel the use of coloured glasses which hamper the geologist more than most. Throughout the year the greater portion of his time is spent on chores of one kind or another.

He spends his time building huts, making and breaking camp, cleaning floors, carrying out routine meteorological observations, cooking—all good for the soul, but bad from the point of view of output of geological work. Geology, indeed, occupies only a very small fraction of his time, though the keen geologist will get unbelievable results by working twenty-four hours a day when the opportunity does arrive.

I take this opportunity to place on record one respect in which I think the old practice was better than the new. I still think that in any expedition that has a heavy scientific programme it would pay to take one man who has a flair for, and can take a pride in, cooking and is willing to take that job on in winter quarters as a main task. I make no apology for mentioning this at a scientific session, for it must vastly affect both the morale of geologists, among other scientists, and their results. That it is possible to combine this with observing and sledging ability the record of Seaman Dickason of Scott's northern party is sufficient proof. A study of the wireless messages from the Royal Society party at Halley Bay suggests that Dalgleish has reached the same conclusion. The point should be of particular importance in the International Geophysical Year when concentration of scientific effort over long periods is inescapable if maximum results are to be obtained.

There are other handicaps under which the Antarctic geologist suffers which I can only mention now. Two out of three sledge parties have perforce no geologist at all. There are other destinations besides geological collections where rock specimens are liable to fetch up. A quite decisive memoir on Antarctic Archaeocyathinae was written upon a cubic inch of limestone breccia that was rescued from the pocket of a shapely souvenir hunter on the deck of a liner by the merest chance. A perfect Archaeocyathus cup disappeared without trace while being passed round in the Scott hut. (On the other hand, a chance dredge haul from the Weddell Sea produced another major monograph and proved the extension of the same formation beyond the continental divide—Dame Fortune's balance tipped the other way.) I have myself hidden a 50-lb. specimen in the bottom of a foodbag we were not likely to broach, to save overworked sailors on a man-hauled sledge from brooding on a thirty-mile journey home. But even laymen can rise to unusual

heights when their enthusiasm has once been aroused, by a geologist they respect. The outstanding example is, of course, Scott's southern party, who retained 40 lb. of priceless specimens through a gruelling march of hundreds of miles and left them carefully labelled on a cairn when their failing bodies could no longer drag them along.

Finally, I would emphasise the great and increasing disparity, for these and other reasons, between the amount of money, man-power and energy put into Antarctic exploration and the scientific results achieved. It is no exceptional matter that this year an American exchange professor working at the Scott Polar Research Institute in Cambridge has as an important part of his task, the description of 'erratics' I collected at Cape Royds forty-nine years ago. Nor is it unusual that today Dr. Campbell Smith will be speaking on unpublished work on Cape Adare based upon specimens I collected there in 1911, whose characteristics I can no longer call to mind. In the past the handicap of the privately sponsored expedition has been lack of cash. Now governments are interested, that trouble has partly disappeared only to be replaced by a subordination of science to politics that has, as the record shows, even graver results. This is an aspect of polar exploration to which, as I have said elsewhere, the British Association, and especially Sections C and E, might well give heed.

In the meantime, so far as this country is concerned, we have endeavoured to deal with the new situation by setting up, with Colonial Office approval and Treasury support, a sub-department in the University of Birmingham whose be-all and end-all is Antarctic geological research. Dr. Adie, who speaks today is entrusted, under Professor Shotton's general direction, with the conduct of this new and promising weapon of Antarctic research.

GEOLOGICAL RESEARCH IN GRAHAM LAND

By Dr. Raymond J. Adie

Graham Land is a finger-like peninsula projecting about 700 miles northward from the mainland of the Antarctic continent in the Falkland Islands Dependencies sector, which lies to the south of South America. Except for the rugged northern region of Trinity Peninsula, the central core of the peninsula is a 6,000–7,000 ft. ice-capped plateau with steep escarpments and coastal mountains on both its west and east sides. The fjord coastline of west Graham Land and the multitudes of offlying islands, resulting from severe glacial dissection, are fairly easy of access from the sea during the summer season, but the greater part of the east coast is fringed by a protective ice-shelf and hence it can only be reached by travelling from the north, across the plateau from the west or from the ice-shelf edge. Exposed rock which occurs mainly along the coastal parts constitutes less than 5 per cent of the total area of the peninsula. The Antarctandean mountain chain of Graham Land is linked with the South American Andes by the island groups forming the Scotia Arc.

Relatively little was known about the geology of Graham Land and the adjacent islands before 1900. With the exception of the Jurassic volcanics and Early Tertiary intrusives discovered on the north-west coast by the Belgian Antarctic Expedition, 1897–9 (Arctowski, 1900; Pelikan, 1909; Sistek, 1912), the only geological specimens brought back from this region were Miocene volcanic rocks and Cretaceous fossils collected by Sir James Clarke Ross's *Erebus* and *Terror* Expedition, 1839–43 (Prior, 1899) and geologically inclined captains of sealing and whaling vessels (Sharman & Newton, 1894–5, 1897–8; Geikie, 1897–8). At the turn of the century, while attention was being directed to exploration in East Antarctica with a view to the conquest of the South Pole, the controversial postulation of a close geological similarity between South America and Graham Land (Suess, 1909)

provoked considerable interest in West Antarctica as a new field for geological research.

The first major contribution to our geological knowledge of this area was that of the Swedish South Polar Expedition, 1901–3, which from its base on Snow Hill Island explored much of the north-east part of the peninsula and the James Ross Island group and carried out important stratigraphical, structural and palaeontological investigations (Andersson, 1906, 1907; Nordenskjöld, 1905, 1910, 1911, 1913). Their work gave us the first detailed accounts of the remarkable Middle Jurassic flora of Hope Bay (Nathorst, 1904, 1907; Hallé, 1913*a*, *b*) and of the abundant Upper Cretaceous and Tertiary faunas and floras of Seymour, Cockburn, Snow Hill and James Ross Islands (Buckman, 1908; Dusén, 1908; Felix, 1909; Gothan, 1908; Hennig, 1911; Holland, 1910; Kilian, 1906; Kilian & Reboul, 1909; Lambert, 1910; Smith-Woodward, 1908; Weller, 1903; Wilckens, 1910, 1912; Wiman, 1905*a*, *b*). The greater part of the petrological collection was unfortunately lost when the *Antarctic* sank in the Weddell Sea and thus prevented the completion of the interesting and valuable work of Baeckström (1915) and Bodman (1916).

The results of the geological work of Dr. Charcot's 'Français', 1903–5 (Gourdon, 1905, 1906, 1907, 1908) and 'Pourquoi Pas?' 1908–10 (Gourdon, 1910, 1914*a*, *b*, *c*, 1917, 1919) Expeditions included the first accounts of the petrology of the Jurassic andesitic volcanics and Anden-granites, diorites and gabbros of the north-west coast and Marguerite Bay.

As a result of prospecting undertaken in 1911 to 1914 on behalf of whaling companies a careful survey of long-known harbours of the South Shetland Islands and the Danco Coast (Ferguson, 1921; Tyrrell, 1921) confirmed Gourdon's earlier investigations. A part of the programme of the Norwegian Antarctic Expeditions, 1927–9, was devoted to the study of the physiography and geology of the South Shetland Islands and the more accessible parts of north-western Graham Land (Holtedahl, 1929; Barth & Holmsen, 1939; Barth, 1940).

Various collections of igneous rocks from the outlying islands and the north-west coast of the peninsula have also been described by Stewart (1937, 1942, 1945, 1947).

Although the greater part of the long-term research programme of the Discovery Investigations was of an oceanographical and marine biological character, important igneous rock collections were made on the west coast of Graham Land and in the South Shetland Islands (Tyrrell, 1945).

The British Graham Land Expedition, 1934–7 was responsible for the early exploration of the south-west coast of the peninsula from its bases at the Argentine and Debenham Islands. Their discovery of Basement Complex metamorphics in Marguerite Bay and the identification of a widespread fossiliferous 'Jurassic' sedimentary succession on the east coast of Alexander Land (Fleming *et al.*, 1938) were two major contributions to the geology of this area. Their work also included important geomorphological and glaciological observations.

The geology of Marguerite Bay and southern Graham Land was further investigated by the United States Antarctic Service Expedition, 1939–41, from a base situated on Stonington Island. The 'greywacke-shale series', already well-known in the Hope Bay area, was found to be intruded by Andean granites and diorites at several localities on the east coast south of Three Slice Nunatak (Knowles, 1945).

The Ronne Antarctic Research Expedition, 1946–8, reoccupied the old U.S.A.S. Expedition base at Stonington Island and continued the geological survey of the Marguerite Bay area. Nichols (1947, 1948, 1955) has described at some length both the general geology and the field relationship between the Basement Complex, the various groups of Palaeozoic and Andean intrusives and the Upper Jurassic rhyolite–andesite volcanics.

Since 1943 the Falkland Islands Dependencies Survey (Wordie, 1946; Bingham,

1947; Mason, 1950; Fuchs, 1951*a*, *b*, 1952), with its many well-equipped bases and adequate resources, has undertaken a long-term programme of exploration throughout the length of the Graham Land peninsula. In addition to the many scientific interests of the Survey, various aspects of the innumerable geological problems posed by previous expeditions have been investigated in the course of routine geological surveys and field mapping (Joyce, 1950; Adie, 1953). During the past twelve years the Survey has been responsible for advancing the overall geological knowledge of Graham Land and the Falkland Islands Dependencies, and it is now possible to present a brief account of the more important results.

A study of the distribution, stratigraphy and petrology of the Archean Basement Complex (Adie, 1954) in Marguerite Bay has revealed a series of amphibolites, *ortho-* and *para*-gneisses, hornblende- and mica-schists, which have been deformed by at least two phases of regional metamorphism. In several localities the Basement Complex is intruded by coarse-grained granites, which have been tentatively assigned to an Early Palaeozoic age (Adie, 1954). Xenoliths of these granites and the Basement Complex metamorphics have been found in association in the Upper Jurassic andesitic agglomerates of the same area. The highly altered and contorted volcanic tuffs of north-east Alexander Land have been tentatively correlated with the earliest volcanics of Pourquoi Pas Island and are also believed to belong to the early Palaeozoic (Adie, 1954). Because of the great distances separating the outcrops of these rocks the exact relationship between the volcanics and the older granites has not yet been established.

The great thickness of almost barren sediments of north-east Graham Land, now referred to as the Trinity Peninsula Series, has been shown to consist not only of greywacke facies sediments but also of arkoses and even quartzites. The age of this series is certainly late Palaeozoic and possibly Carboniferous. In pre-Jurassic times these strata suffered cataclastic deformation which transformed the argillaceous beds into contorted slates and severely shattered the arenaceous beds. In Trinity Peninsula and elsewhere along the Graham Land east coast these strata are marginal to the Andean intrusions, which have been responsible for the contact metamorphism (Adie, 1957). In the thermal aureoles surrounding the smaller granite and diorite intrusions of Trinity Peninsula, hornfelses of widely differing textures and mineralogical compositions but often retaining their original slaty cleavage have been recorded. Petrological studies on these hornfelses and slates have provided a means of detailed correlation with the type area in Trinity Peninsula. From recent field information it is believed that the most highly metamorphosed equivalents of the Trinity Peninsula Series are present on the Danco Coast.

The Middle Jurassic basal conglomerates and plant beds of Mount Flora (Hope Bay), which were discovered by Andersson, are now known to extend along the east coast of Trinity Peninsula as far south as Cape Longing and Cape Sobral. In the former locality the Upper Cretaceous beds overlie the Jurassic plant-bearing grits unconformably. However, at Mount Flora there is a gradual upward transition from lake sediments into rhyolitic tuffs and even flow lavas. The lateral equivalents of these rhyolites have been mapped at many places along the east coast as far south as Churchill Peninsula and Jason Island. The total thickness of the volcanic sequence has been estimated at almost 8,000 ft. At Cape Disappointment variegated green, pink and bright red, 'pyjama-striped' rhyolitic tuffs, showing remarkable micro-structures, contain beds of carbonised woods (Adie, 1952), thus indicating that former forests were inundated by these volcanic outbursts. It is now clear that the 'quartz-porphyries' described by Andersson (1906) are really rhyodacitic crystal tuffs, which form a prominent part of the volcanic succession. Although they have been recorded in several localities, andesites are not common in the east coast sequence.

The Upper Jurassic andesite–rhyolite volcanic group apparently reached its

maximum development in the Marguerite Bay area, where the classical basic to acid evolutionary trend in a volcanic cycle is well demonstrated. Extrusion centres have been discovered at a number of localities and from available data it has been established that there was a southerly trend in volcanic activity with the passage of time.

While volcanism was proceeding with some vigour on the north-west coast and in Marguerite Bay, Alexander Land was the scene of active sedimentation. A considerable amount of air- and water-borne volcanic material derived from the explosive phases of the more northerly volcanism reached this area and preserved belemnites, ammonites (Howarth, 1957), gastropods and lamellibranchs in an aqueous tuff matrix. In Upper Jurassic times a great thickness of sediments accumulated in this region. Although it was previously considered there was a conformable transition into the Lower Cretaceous, it is now certain that the 800 ft. conglomerate discovered at the base of the Cretaceous (Adie, 1952) indicates an unconformity or disconformity. This is confirmed by the presence of an abundant molluscan fauna of Aptian age in the sediments above the conglomerate (Cox, 1953; Howarth, 1957). These beds also contain plant material. Later tectonic disturbances, responsible for the formation of the 10,000-ft. Douglas Range of north-east Alexander Land, have caused considerable faulting and overthrusting of these sediments. Because of the complex nature of the structural and tectonic pattern the overall picture has not yet been completely worked out.

In north-east Graham Land and the James Ross Island group the Upper Cretaceous (Lower and Middle Campanian) beds, which comprise a succession of interbedded coarse sandstones, shales, glauconitic sandstones and calcareous sediments with concretions, have provided the palaeontologists with an abundant molluscan fauna (Spath, 1953; Howarth, 1957) and even foraminifera. Plant remains, which in a few places have given rise to thin coal seams, have also provided valuable information on climatic conditions. One of the most interesting discoveries in recent years has been that of a fossil vertebra which has been tentatively identified as belonging to a Plesiosaur-like reptile, the first to be recorded from Antarctica.

The contact between the Upper Cretaceous beds and the Mid-Tertiary (Upper Oligocene to Lower Miocene) mudstones and sandy shales of Seymour Island, which was previously thought to be a disconformity, has now been established as an unconformity. Associated with brachiopods, lamellibranchs and plants are the fossil remains of gigantic penguins, which have been assigned to a Lower Miocene age (Marples, 1953). These beds have also yielded vertebrae similar to those found by Nordenskjöld and which are believed to belong to an Archeocete (fossil whale).

In north-east Graham Land and the South Shetland Islands the Middle to Upper Miocene was a period of volcanism and there were at least two lava and three explosive phases. Palagonite tuffs, agglomerates, breccias and highly vesicular olivine-basalt lavas of the James Ross Island Volcanics poured forth over a wide area from a number of eruption centres, many of which have now been located. In the South Shetland Islands, where they were deposited in water, the tuffs are ripple-marked and current-bedded and contain perfectly preserved plant remains.

The Pliocene (? early Pleistocene), represented by the Pecten Conglomerate, has only been found as a small outcrop resting on the James Ross Island Volcanics at Cockburn Island about 1,000 ft. above sea-level.

Undoubtedly the most important group among the Graham Land rocks is the Andean Granite–Gabbro Intrusive Suite, which forms at least 90 per cent of the peninsula. Although it has not yet been possible to establish the precise age of these intrusives, they have been referred to an Early Tertiary age on the basis of petrological and geochemical analogy with the Andean intrusives of the West Patagonian Cordillera. The Late Cretaceous–Early Tertiary was a period of large-scale batholithic emplacement which accompanied the major episode of the Andean

Foldings and it was at this time that Graham Land first made its appearance as a true peninsula. After a detailed examination of the various phases of intrusion it has been possible to correlate the different stages from one area to another. From geochemical and petrological studies it has been shown that these intrusives are a true crystallisation differentiation series derived from a magma, the composition of which has been calculated (Adie, 1955). Migmatisation phenomena and contamination of the parental magma have been found to be of less significance than previously supposed.

In the course of their emplacement as small boss-like masses and as large batholiths the Andean intrusives have thermally metamorphosed both the Trinity Peninsula Series sediments (Adie, 1957) and the Upper Jurassic Rhyolite–Andesite Volcanic Suite (Adie, 1953).

All these investigations have been directed primarily towards the elucidation of the geological history and structure of the Graham Land peninsula and the adjacent islands. In spite of the work already accomplished in this area, there still remain a multitude of geological problems to be solved. However, it now seems probable that the Andean chain, which extends into Graham Land, crosses West Antarctica and links with the mountain ranges of King Edward VII Land and even perhaps New Zealand.

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SOME GEOGRAPHICAL RESULTS OF THE WORK OF THE FALKLAND ISLANDS DEPENDENCIES SURVEY, 1944–55.

By Dr. D. H. Maling

A comparison of Antarctic research during the last fifteen years with the work carried out before 1939 shows that the most noteworthy features of the post-war period have been the size and continuity of the various enterprises. Fig. 1 shows the number of separate parties which have wintered in the Antarctic or on adjacent islands during the period 1898–1955.

One of the most important contributions to this post-war activity has been the work of the Falkland Islands Dependencies Survey in the Graham Land sector of Antarctica.

The Falkland Islands Dependencies (Fig. 2) comprise those parts of the Antarctic continent which lie between longitude 20° W. and 80° W., together with the islands of South Georgia, the South Orkneys, South Shetlands and South Sandwich group and the islands close to the Graham Land peninsula.

The present venture began as a small naval operation during the war. The first base, at Deception Island, was established on February 6, 1944. Since that date, the activities of the Survey have been continuous. After the war, the Falkland Islands Dependencies Survey was given its present title and the administration of the project was transferred to the Colonial government (1).

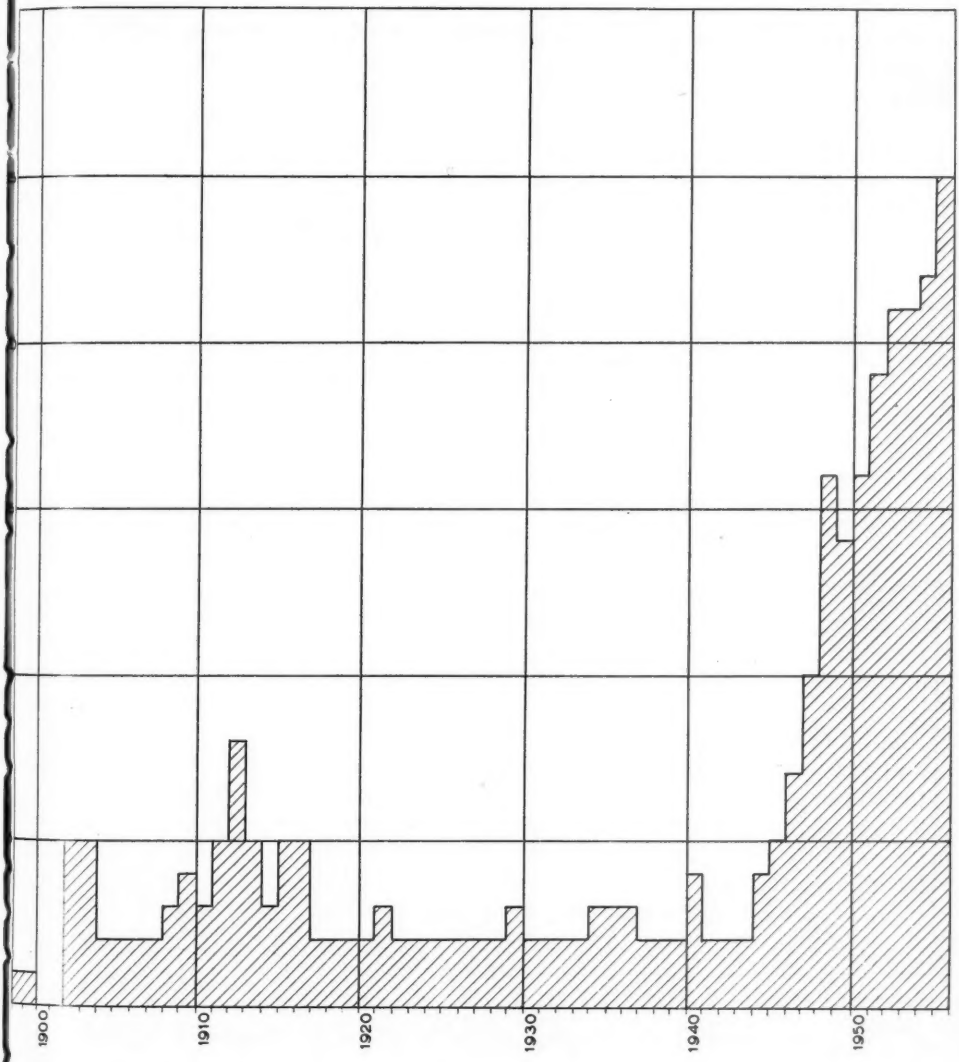
The Survey has two main objectives:

(1) It is responsible for the maintenance of the administration of H.M. Government in the Falkland Islands Dependencies.
(2) It carries out scientific investigation of the potential resources of these territories.

The Survey is financed from the revenue of the Dependencies—largely from the taxes derived from the whaling and sealing industries which are based on South Georgia; partly from lesser items such as the sale of stamps. The Scientific Bureau, in London, has received a grant under the Colonial Development and Welfare Scheme.

During the early years, the Survey grew rapidly in size and activity (2, 3). Between 1950 and 1953, the work was largely confined to routine meteorological observations, but a second phase of growth, which still continues, began in 1954. Since it is too early to assess the value of the work carried out during this later period, the present discussion is limited to the activities of the shore parties which have wintered at the nine bases established before 1954. Of these, only one, Hope Bay, is actually situated on the mainland of Antarctica, though the coast of Graham

Land is always accessible from Stonington Island, and, probably, from the recently established base on Horseshoe Island. The other bases are situated on relatively small islands and, in most places, the opportunities for travelling are limited by the security of the winter ice. Consequently, in the years when the mainland bases

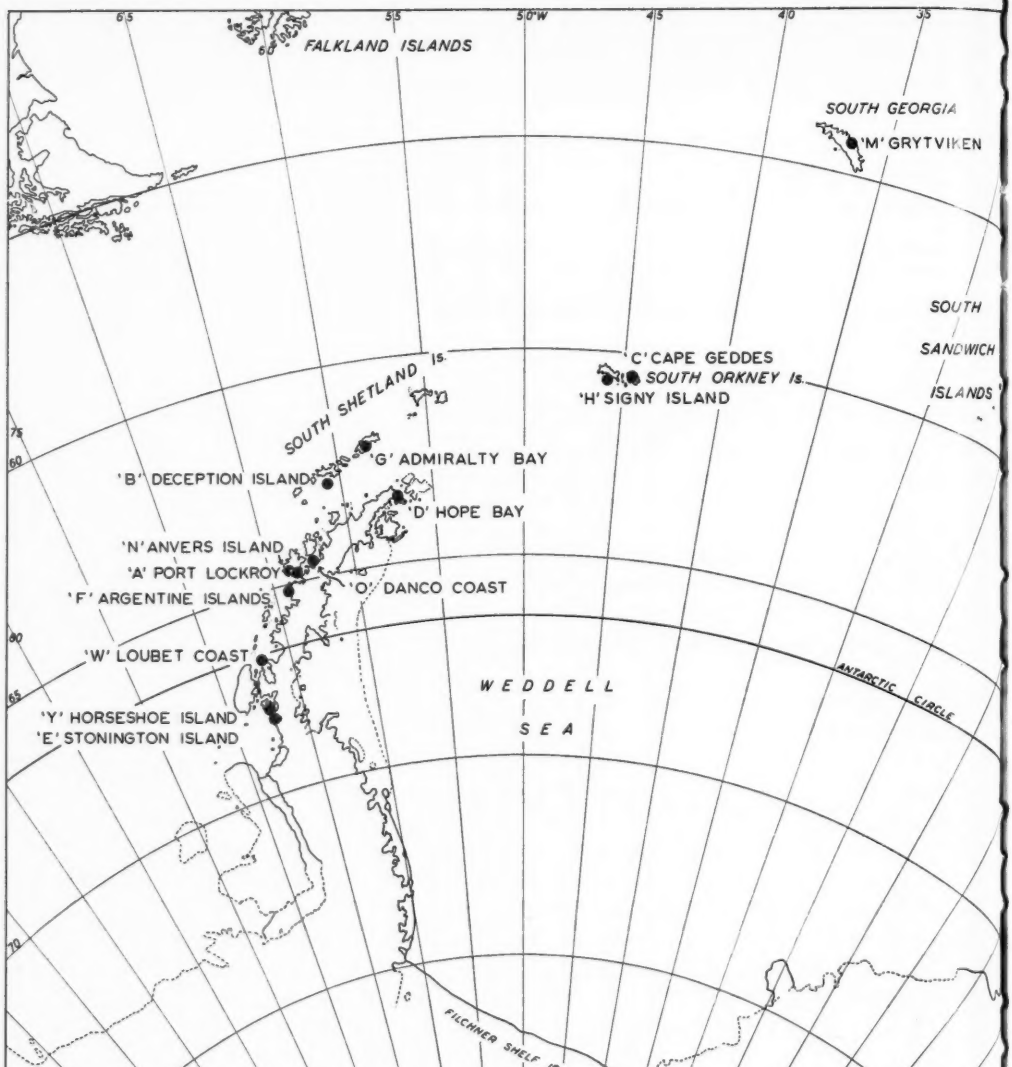


UMBER OF SEPARATE PARTIES WHICH HAVE WINTERED IN ANTARCTICA AND ADJACENT ISLANDS 1898–1955.

Fig. 1.

were occupied, the emphasis there was for extensive exploration by travelling, whereas the parties at the small bases on isolated islands have been principally concerned with a static life and making routine observations.

With the exception of 1948, when only three of us wintered at Signy Island, the minimum number at a base has been four. Before 1951, this was the normal strength of the smaller bases; today the usual number is six. The large bases have



No. OF WINTER PARTY	1944	1945	1946	1947	1948	1949	1950	1951	1952	1953	1954	1955	1956
PORT LOCKROY	9	4	4		4		4		5	5	5	4	5
DECEPTION ISLAND	5	4	4	5	4	4	4	4	6	5	5	6	6
CAPE GEDDES			4										
HOPE BAY		13	8	9	7				11	10	12	12	12
STONINGTON ISLAND			10	11	11	11							
ARGENTINE ISLANDS				4	4	4	4	5	4	5	10	9	9
ADMIRALTY BAY					5	6	4	5	5	5	5	5	6
SIGNY ISLAND				4	3	4	5	5	5	5	5	7	8
GRYTVIKEN							(5)	(5)	(3)	(2)	(6)	x	x
ANVERS ISLAND												6	6
DANCO COAST													6
LOUBET COAST													8
HORSESHOE ISLAND												8	10

Fig. 2.

been manned by seven to thirteen people. The average party is eleven men. Each base has its nucleus of essential people; a meteorologist, a radio-operator and, in recent years, a diesel-electric mechanic. The large bases generally have a medical officer, a geologist and one or two surveyors. Other specialists such as biologists, dog physiologist and glaciologist have been appointed to the Survey from time to time.

The different bases have tended to develop specialised activities. For example, the drowned caldera of Deception Island forms one of the best deep-water harbours in the whole of Antarctica, with the additional advantage of a relatively long ice-free season. Moreover, it is conveniently situated for access to the other bases in the South Shetlands and Graham Land. It provides a natural centre from which movements of shipping can be conducted in summer, where a vessel can await improvements in sea-ice conditions at other bases and where stores and personnel can be left between voyages. The significance of this 'capital' of the Dependencies is shown by the presence of three permanent bases, Argentine, British and Chilean, on this small island. The Argentine Islands were used for many years as a centre for dog breeding and training because the supply of seals was better than at the other Graham Land bases. This base has now become the principal geophysical centre where problems of terrestrial magnetism, gravity and upper air meteorology can be studied. Signy Island, in the South Orkneys, has a particularly rich animal life so that this has been the principal centre for zoological research. The base at Grytviken is differently organised, for the F.I.D.S. staff have been absorbed into the residential administrative population and the most important work is the provision of weather forecasts during the whaling season.

The personnel are recruited in the United Kingdom by the Crown Agents. In the past, a fair proportion of the 'Handymen' were recruited in the Falkland Islands, but, today, there are few Falkland Islanders in the wintering parties. The tour of duty is either one winter or two. A few people have joined the Survey for further periods. Four men have spent four winters with the Survey: one man has spent five winters at different bases.

The entire work of the Survey, up to the end of the winter of 1954, may be expressed as 339 man/years. In Fig. 3 this effort has been divided according to the size and duration of each base and further subdivided according to the representation of different scientists. The manuscript records of work done at the bases are passed to the F.I.D.S. Scientific Bureau. There are now about 1,100 separate reports covering different aspects of life and work at the bases. In general, those which are of value to the present discussion may be classified as follows:—

(1) Routine observations of meteorology, sea-ice conditions and animal life, made at each base.
(2) General investigations made on journeys.
(3) Special investigations carried out by a scientist at one base.

We must select the routine meteorological work as having prime importance in the scientific work of the Survey.

There are two reasons why these results are outstanding. First, they provide some of the longest records of continuous observations in high southern latitudes; secondly, they constitute the longest series of simultaneous observations from closely grouped bases in Antarctica.

Apart from the well-known series of meteorological observations made at Laurie Island and South Georgia, where the stations have been maintained for fifty-two and fifty-one years respectively, none of the older Antarctic climatic records were of more than three years' duration. This has formed one of the greatest deficiencies in our knowledge of Antarctic climatology, for the earlier records gave little idea of the range or variability of climate. Meteorological observations have now been

FALKLAND ISLANDS DEPENDENCIES SURVEY
1944 - 1954
TOTAL PERSONNEL
METEOROLOGISTS
SURVEYORS
GEOLOGISTS
PHYSICISTS
GLACIOLOGISTS
ZOOLOGISTS & BOTANIST
PHYSIOLOGISTS
A PORT LOCKROY, PALMER ARCHIPELAGO 64°50'S 63°31'W.
B DECEPTION ISLAND, SOUTH SHETLAND Is. 62°59'S 60°34'W
C CAPE GEDDES, LAURIE I SOUTH ORKNEY Is. 60°42'S 44°34'W.
D HOPE BAY, TRINITY PENINSULA 63°24'S 56°59'W
E STONINGTON ISLAND, MARGUERITE BAY 68°11'S 67°00'W
F ARGENTINE ISLANDS, 65°15'S 64°16'W
G ADMIRALTY BAY, KING GEORGE ISLAND, S SHETLAND Is 62°05'S 58°25'W
H SIGNY ISLAND, SOUTH ORKNEY ISLANDS 60°43'S 45°36'W
M GRYTVIKEN, SOUTH GEORGIA 54°16'S 36°30'W
100
50
25
10

Fig. 3.

made at Deception Island for 13 years [1] and at three other bases for 9 years. We have, in consequence, a more accurate knowledge of the climate of the South Shetland Islands and western Graham Land where, formerly, there were only isolated records for one year or less (4).

Secondly, since this post-war interest in Antarctica has not been confined to the Falkland Islands Dependencies, there are concurrent records from the other bases established for different periods in Antarctica and from the semi-permanent stations on Heard Island, Marion Island, Îles de Kerguelen and Macquarie Island. The result of this increase in synoptic data has been a considerable advance in the application of frontal analysis to forecasting, and in our knowledge of the general circulation in high southern latitudes.

Associated with the problems of synoptic meteorology are those of regime of sea-ice round the coasts of Antarctica. The movements of pack-ice and the duration of fast-ice are of particular importance to navigation; the extent of the ice-covered sea may modify the surface layers of air masses and the position of the ice-edge is alleged, by some, to guide the movements of pressure systems. During the inter-war period, when whaling fleets were particularly active in the Falkland Islands Dependencies, and the ships of Discovery Investigations were frequently in these waters, a great deal of information about the summer movements of pack-ice was accumulated (5). At this time, however, the usual regime of winter ice-cover was uncertain, for few parties had wintered along the Graham Land coasts, observations were only available for isolated winters and these showed considerable variation. The routine observations carried out at the F.I.D.S. bases have great potential value, but detailed study of the records is not yet complete. The observations are restricted to the visible horizon so that, although the seasonal movements and variability of ice along west Graham Land are known, we still have little idea about the winter distribution of pack-ice in the open sea. Additional observations have been made during the course of sledge journeys and aircraft flights but these are isolated records. Only regular flights across the Weddell and Bellingshausen Seas can really provide the synoptic picture of ice distribution which is desirable.

The small amount of systematic glaciological research is deplorable. With the exception of 1949, when a glaciologist worked at Admiralty Bay and the present writer made ablation measurements at Signy Island, hardly any studies of the glacier regime have been attempted. Routine measurements could easily be made at every base, yet there are hardly any quantitative data for any other bases or years. Most geologists are agreed that the ice-forms of Graham Land and the islands show signs of recent and rapid retreat, but preliminary investigations of the regime at Signy Island suggest that, in 1949, the island ice-cap was almost in equilibrium. The investigations of the Norwegian-British-Swedish Antarctic Expedition in Dronning Maud Land suggest that similar conditions prevail on the margin of the continental ice (6). It is proposed that glaciologists should work at certain bases during the International Geophysical Year, but it is a matter for regret that this work was not begun ten years ago.

Topographical surveying has been carried out at all the bases with the exception of Cape Geddes and the Argentine Islands. During the period that F.I.D.S. parties have been active, the majority of the work has been done from the bases at Hope Bay and Stonington Island. There have been three major sledge journeys which have covered completely new ground, when F.I.D.S. surveyors were able to make important contributions to the mapping of Antarctica.

In 1946, a party from Stonington Island, under the leadership of Dr. R. S. Slessor, reached the summit of the Graham Land plateau and travelled northwards to latitude 66° 29′ S. This journey showed that although the plateau rises steeply to 6,000 ft. from the Fallières Coast, its width is seldom more than 10–15 miles.

[1] Some early records were lost in 1946 when the original hut was destroyed by fire.

The following year, a party from Hope Bay, under the leadership of F. K. Elliott, travelled southwards, along the Larsen Shelf Ice and crossed Graham Land to Stonington Island. This journey of 700 miles had been the ambition of several expeditions since Nordenskjöld visited the northern part of the ice shelf in 1901–1903. As a result of this journey, the broad outlines of the eastern coast of Graham Land were determined for the first time.

In 1947, also, a joint British and American party, from the two bases at Stonington Island, crossed the peninsula to the Larsen Ice Shelf and travelled southwards to latitude 74° 40′ S. near the western extremity of the Filchner Ice Shelf. This coast had been seen from the air in 1940, but during this 1,200 mile journey, the topography of the eastern side of the pensinsula was accurately surveyed from the ground.

These three journeys, in two successive seasons, made more outstanding contributions to our knowledge of the topography of the mainland than any later work. The journeys from Stonington Island after 1947 had greater geological value; those from Hope Bay have filled many gaps in the survey of the north-eastern part of the Graham Land peninsula. Elsewhere the work has largely consisted of extending inland the pre-war surveys of Discovery Investigations, the British Graham Land Expedition and other parties. The Directorate of Colonial Surveys is responsible for the publication of the completed maps (7).

Despite this activity, there are still considerable parts of the Dependencies which have not been examined in adequate detail. Apart from the far southern latitudes and the eastern coast of the Weddell Sea, which have been beyond the range of this post-war activity, there is still about 200 miles of the west coast of Graham Land which is not properly surveyed. Many of the offshore islands are little known also. Most of the unsurveyed coasts are precipitous and surrounded by fast currents and tidal streams which prevent fast-ice from remaining secure during winter. The current work carried out by Hunting Aerosurveys Ltd., from Deception Island, during summer, is intended to provide aerial photographs of part of this territory at least. It is too soon to say what results will be obtained from this venture, but it appears, at present, that the flights which were made during the first season had only limited success (8).

The major distinction between the work of the Survey and that of the principal pre-war expeditions has been the concentration upon continuous routine observations and careful systematic investigation in the better-known parts of the Dependencies. It is important to remember that, apart from a few hundred miles of eastern Graham Land, virtually every part of the Dependencies where F.I.D.S. parties have been working had been seen from a ship or from the air before 1940.

To a certain extent this policy has been dictated by the political necessity of maintaining continuous residence, but, from a scientific point of view, such a policy is most desirable for the continuity of routine observations cannot be achieved in any other way. In this sense much of the work of the Survey has the same monotonous unspectacular quality as the patient marine research of Discovery Investigations between the wars.

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WINDS AND WEATHER IN THE ANTARCTIC

By H. H. Lamb

Introductory: Former Theoretical Ideas

The history of theories about winds and weather over Antarctica is a nice example of the sort of seesaw between observation and theory by which Man commonly arrives at scientific understanding. First we have a few observations, then a theory to explain them. Later observations produce some discrepancies, and sooner or later the theory is amended. This process of successive approximations goes on until a workable insight is gained that is sufficient for the needs of the time.

The first plentiful weather observations from latitudes south of 40° S. came from the sailing vessels regularly plying their route from the old southern colonial countries to Europe in the eighteenth and nineteenth centuries, using the Brave West Winds and sometimes going as far south as 55 to 70° S. The American oceanographer and meteorologist Maury (1) extrapolated these prevailing west winds through another thirty degrees of latitude to the South Pole and assumed a generally cyclonic wind circulation about the Pole.

As one sails south, however, through the endlessly overcast, gloomy skies of the stormy Southern Ocean one comes eventually to a region close to the ice and to the coasts of Antarctica where clear skies become common and easterly winds prevail. All the early voyages of exploration, by Cook, Bellingshausen, Biscoe, Ross and Dumont d'Urville, made some encounter with these conditions.

In 1882 Ferrel (2) managed to fit these observations into a theory of the general circulation, which is still broadly acceptable as a description of the *average* state of affairs. He wrote:

> Near the poles the tendency [of the air] to flow towards the equator . . . causes a current there which [being deflected by the rotation of the earth] causes a slight NE. wind in the northern frigid zone and a SE. wind in the southern frigid zone; but this is only near the earth's surface, and the general tendency of the atmosphere in the upper levels must be towards the east [i.e. upper westerly winds].

There followed some interesting sentences, which were, however, wrong in detail, about what should happen where the outflowing polar air met air of warmer origin at the surface 'near the polar circle'.

Thus, the idea of a prevailing anticyclonic regime at the surface over the polar caps was born to account for the frequent clear skies and outflowing, easterly winds near the coasts of Greenland and Antarctica.

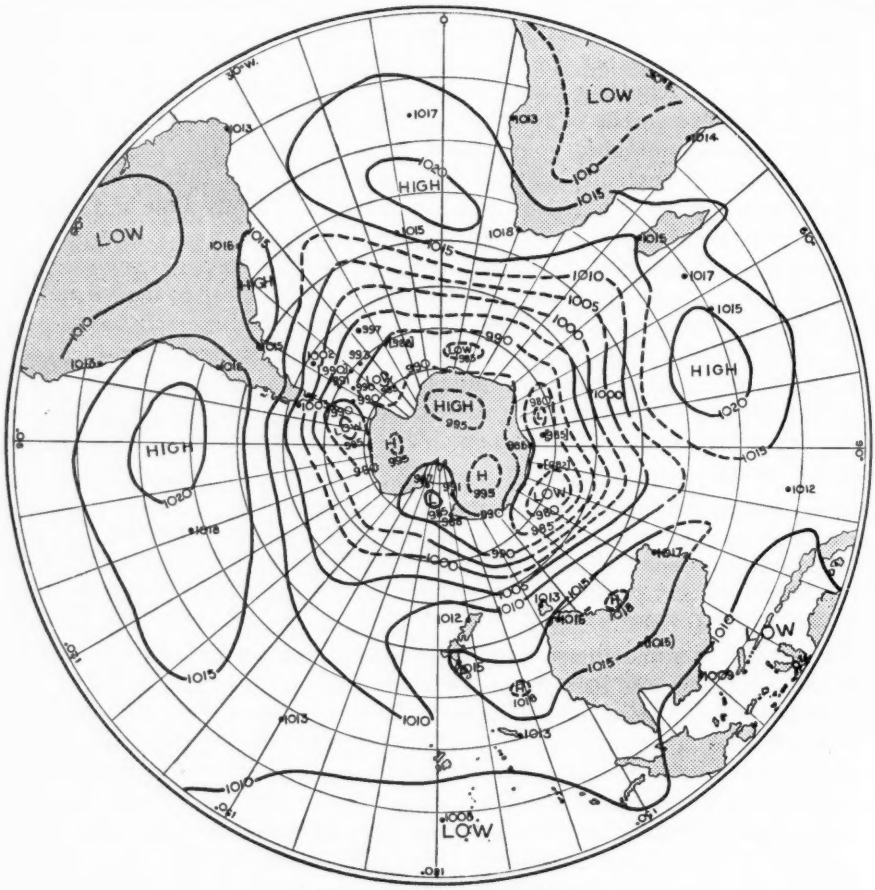
These winds and weather were on the whole confirmed by early expeditions to the interior of Greenland and by the first expeditions in Antarctica in the era 1899–1914. At one or two expedition bases on the coast of Antarctica in certain months winds from an easterly point occurred at over 90 per cent of the time in certain months and in the ascent of the Ferrar glacier and sloping parts of the fringe of the ice-cap down-slope (katabatic) surface winds were observed with great constancy.

From 1910 onwards an American professor of geology, W. H. Hobbs, expounded his theory of *permanent* glacial anticyclones over the ice-caps of Greenland and Antarctica (3), (4), (5). In an introduction to Hobbs's work Hugh Robert Mill wrote in 1926:

> . . . successive theories of world wind circulation . . . all assumed the polar regions to be areas of low sea-level pressure, fed by inblowing winds. . . . In the case of the Antarctic this was possible only by ignoring the observations made in high latitudes by (the early voyagers). . . . Absolutely consistent observations by Antarctic explorers of the twentieth century confirmed the observations and contradicted the theories of the nineteenth.

Meteorologists at first received Hobbs's suggestions in a critical spirit, particularly on account of the theoretical difficulty felt in explaining the occurrence of a great ice-cap in a permanently anticyclonic region with constant descending and therefore dry winds. Hobbs made some effort to get over this objection, really by an impossible construction. His theory won surprisingly general acceptance for a generation, and still dominates the thinking of some who approach the Antarctic today.

However, Hobbs in his turn had ignored some of the observations—for example, of Nansen (6) in Greenland who had sixteen days with snow or rain out of forty



[*By courtesy of H.M.S.O., London*]

Fig. 1.—*Suggested Annual Mean Pressure Pattern over the Southern Hemisphere.*
Figures in brackets taken from the 1946–47 whaling vessels' observations which happen to appear reasonably indicative of probable yearly values over the same parts of the Southern Ocean.

days spent in the first crossing of the inland ice in August and September 1888, and of the Antarctic expeditions of the early years of this century. In the case of David (7) who experienced a period of north-westerly winds blowing up the slope of the ice-cap near the South Magnetic Pole in 1909 it appears that Hobbs wittingly or unwittingly falsified the awkward observations perhaps by too hasty reading of David's narrative. Hobbs's theory has long since been shown to be in conflict with observation in Greenland (8), where the ice-cap is nourished by ordinary snowfall in cyclonic conditions, especially in the southern and central parts which are the highest. Now it is our business to amend Hobbs's interpretation of the anticyclonic regime seen on the map of average pressure over Antarctica (Fig. 1).

Recent Empirical Work Based on Synoptic Weather Maps

In changing the words 'prevailing' and 'predominant' in the explorers' accounts to 'permanent' Hobbs had seriously overstated the case for the Antarctic anticyclone, even though in some places easterly winds are heavily predominant. The concept of permanency is no more tenable in relation to Antarctic anticyclones than for other atmospheric circulation systems elsewhere.

This much has become clear within the last ten years, which have seen the establishment of a limited number of permanent bases on the coast of Antarctica and on widely scattered islands in the Southern Ocean. The weather observations from these places have made it possible for the first time to construct daily more or less complete maps of the circulation patterns over the southern hemisphere and to trace the interplay of systems in different latitudes. Particular interest is attached to the continuing series of daily synoptic weather maps prepared from about 1951 onwards under the Southern Hemisphere Weather Analysis Project at the Weather Bureau, Pretoria.

These maps have made it clear that

(i) the main depression tracks are over the ocean and off the coast of Antarctica in subpolar latitudes (Fig. 2). The average M.S.L. pressure of the atmosphere in this depression belt is lower than anywhere else on earth, but the pressure gradients (and therefore the winds) are often quite weak over extensive areas in the central regions of the depressions. Intense centres with strong pressure gradients are marked by considerable departures from the normal pressure for the region. Thus in a region where the annual mean pressure is between 980 and 990 mb. very many depressions have central pressures of 960 to 970 mb. Intense depressions have brought M.S.L. pressure down to 927 mb. on the coast of Adélie Land and to 932 mb. on the Ross Ice Shelf at the head of the frozen Ross Sea in winter.

(ii) pressure is predominantly rather higher over Antarctica with a generally anticyclonic distribution of (anticlockwise) outblowing surface winds (mostly easterly and south-easterly). On many days, however, this is merely a region of very limited extent with slack circulation between the surrounding depressions. When there is a well-defined anticyclone over Antarctica, this may be centred in almost any position, but the broadest region of the continent between the Pole and Enderby Land (say between 80 and 85° S. and 20 to 100° E.) is the commonest part (see Fig. 2).

Cases have been noted in which the main centre of high pressure was right out over the Weddell Sea or had considerable extensions over the Ross or Bellingshausen Seas. Such eccentric positions in winter must favour unusual production of ice in the sea areas concerned, though it is likely that the polar anticyclone seldom thrusts out beyond the edge of the pack ice for long. There is no evidence of very high pressures comparable with northern hemisphere extremes ever occurring over Antarctica. Sea-level pressures at coastal bases are commonly 1,000 to 1,005 mb., and sometimes as low as 985 to 990 mb., in high-pressure systems emerging from the Antarctic interior. The identity of these systems is soon lost when they pass out over the ocean.

(iii) the highest pressures observed in Antarctica (1,030 to 1,036 mb. respectively on the outer coast opposite Australia and in the Graham Land peninsula) have all occurred in so-called 'blocking anticyclones' which developed towards high latitudes from the subtropical anticyclone belt. These blocking systems are much rarer over the Southern Ocean than they are in the northern hemisphere and are almost confined to the colder

months. These and some of the anticyclones originating over Antarctica have the subsiding cores and raised tropopause of 'warm' highs, and the anticyclonic circulation extends up into the stratosphere.

(iv) much of Antarctica commonly comes under cyclonic influence from the bigger oceanic lows and occasionally depression centres pass inland. The sector south of Australia and a relatively low-lying region S.E. of the Ross Sea are probably most affected by depressions passing over the ice and

[*By courtesy of the Royal Geographical Society*]

Fig. 2.—*Depression frequencies: Jan.–Feb. 1947, mid March–mid April 1947, mid March–mid April 1948.*

Mean monthly positions of the centres of anticyclones over Antarctica for each month Jan. 1951–Nov. 1953 shown by dots.

sometimes centred over the ice for days, or even weeks, together. The travelling depressions are mostly frontal systems with a tongue of warm air aloft sufficiently far south to give a well-marked steering current (thermal wind) over the depression. The more lingering depressions are fully occluded, cold centres in which the cyclonic circulation is developed up to great heights, though the centres become weak and sluggish (rather light winds) at the surface over the Antarctic ice.

We recognise both warm and cold types of anticyclone and depression in the Antarctic. The wind circulation of all well-marked high and low pressure systems

extends up to at least the level of the higher parts of the ice cap, which are liable to be swept by the accompanying gales. The situation most conducive to really strong winds is when there is a strong pressure gradient between well-marked anticyclones and depressions in juxtaposition—here as in other parts of the world. To this extent gales are more liable to occur when there is an anticyclone over part of Antarctica than in periods of generally low pressure when there is no anticyclone.

These suggestions of our experience with synoptic weather maps would sound revolutionary to Hobbs, but actually they encourage the belief that at last our theoretical standpoint can comprehend all the observations.

What it amounts to is that most of the time an (often weak) anticyclonic regime with katabatic winds which become easterly and south-easterly near the coast, prevails over most of Antarctica. Taken over a long period this is probably true over East Antarctica for 85 per cent of the time. But we must reckon with radically different patterns and weather for part of the time, there being some fairly long spells of abnormal weather and circulation patterns.

Actually, this should have been obvious from the reports of early explorers. There is an obvious predominance of clear skies and anticyclonic type over the ice-cap, but important variations do occur and some areas are subject to frequent gales and blizzards. Overcast skies and snowfall also occur.

Amundsen had snowfall from altrostratus cloud on the South Polar Plateau and evidently frontal wind changes on three occasions in the forty-six days he was there. On five occasions the party was enveloped in mist on the plateau.

Shackleton, Scott and Amundsen all had some experience of gales and blizzards on the Polar Plateau.

Several other sledge travellers in the interior of the sector south of Australia have described obvious frontal sequences with cloud belts coming up, commonly from the *south* and *south-east* in this sector, giving a period of snow and passing over as the wind changed—for example, Mawson's report on November 17, 1912, at 67½° S. 143° E. (9).

Scott was brought to a standstill on two early journeys, high on the polar plateau and west of the Victoria Land mountains, by skies overcast with thick altostratus which rendered navigation impossible.

The first observations received from one of the present advanced expedition bases in the interior, the Soviet base Pionerskaya at 8,000 ft. in 70° S. 95° E., 250 miles south of the coast of the Indian Ocean sector, for a week in June 1956 include two similar spells of overcast due to altostratus cloud (here believed to have spread from the north).

Estimates of Frequency and Duration of Various Kinds of Bad Weather in Antarctica

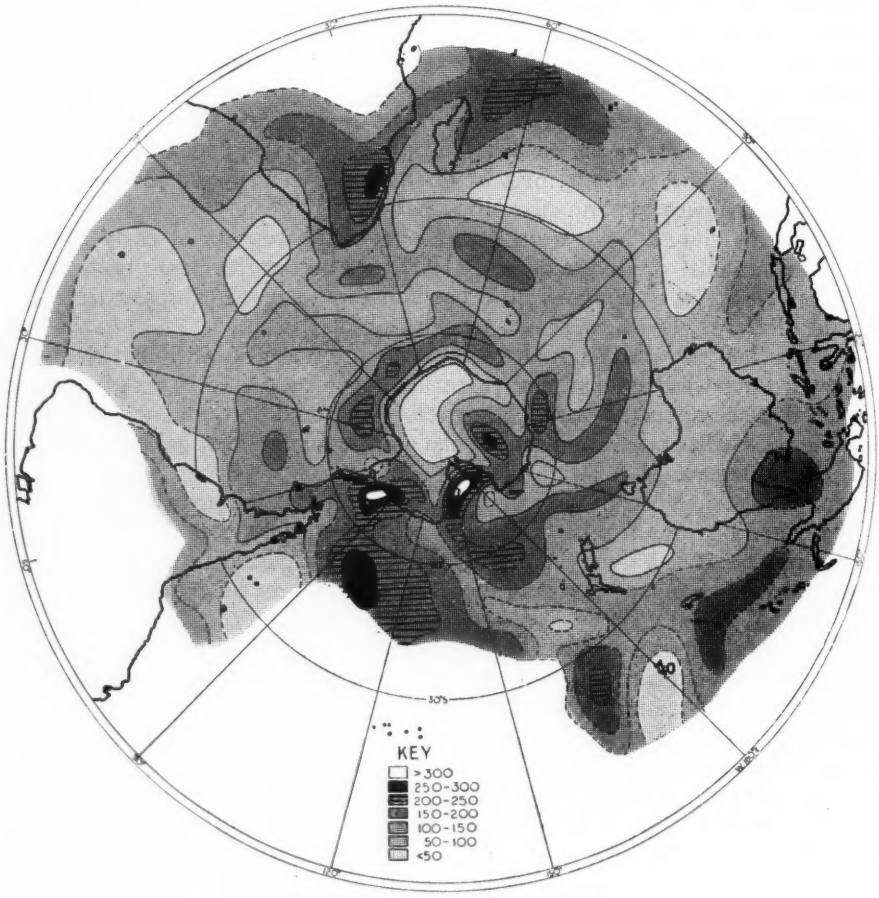
It becomes important to attempt some estimate of the frequency and duration of bad weather conditions in different parts of Antarctica.

(1) *Frontal weather*

Fig. 3 depicts the distribution of frequency of occurrence of fronts during one sample month, March 1947—the first month for which sufficiently complete daily weather maps came to hand as a basis for this statistic. Numerous observations from sledge parties in the interior (9), (10), (11) and observers at the coast (12) show that fronts over Antarctica are accompanied by belts of medium cloud and often by snowfall and occasionally by clouds enveloping the ice-cap surface for a time; typically abrupt changes of wind also occur. The month shown in Fig. 3 was a notably cyclonic one over the Australian sector of Antarctica (with fronts over the

continent on about half the days of the month) and no anticyclone was present south of the polar circle on most days. Even so, the broadest part of East Antarctica was nearly immune to frontal activity. Most places in this region had only one to two days in the month with any kind of front within 200 miles.

The sector between 0 and 90° E. probably holds the fairest weather routes to the South Pole, whatever the geographical difficulties of the long route might be. Moreover, between 20° W. and 20° E. the prevailing wind-flow around the Antarctic



[*By the courtesy of H.M.S.O., London*]

Fig. 3.—*Frontal frequencies in the southern hemisphere, March 1947.*

Mean number of fronts occurring per day within an area 100 × 100 square nautical miles: the numbers in the key should be divided by 1,000.

anticyclones in their favourite position over East Antarctica tends to oppose the katabatic drainage of cold air off the ice-cap, reduce the frequency of gales and offer the best hope of favourable winds to expedition parties making for the interior.

(2) *Cyclonic and anticyclonic spells: evidence of pressure values*

A persistent cyclonic situation over the Ross Sea and Australian sector lasted throughout the first three weeks of March 1947. We find that such long spells, sometimes of anomalous character, lasting up to two or even three months in extreme cases, are a feature of the climate of Antarctica. The wet and dry summers,

severe and mild winters of northern Europe are a similar phenomenon. Examples of spells which affected the pressure of a whole month or longer are easily found, especially amongst the colder months, as illustrated by the following table:

MONTHS WITH HIGH AND LOW PRESSURE

Place	Month	Mean Pressure at M.S.L. for the Month	Departure from the Average of all Available Years
MacMurdo Sound 78° S. 166° E.	June 1903	1,001·1 mb.	+ 13·4 mb.
	June 1912	978·0 mb.	— 9·7 mb.
Bay of Whales 78° S. 164° W.	November 1911	1,000·0 mb.	+ 11·3 mb.
	November 1934	973·1 mb.	— 15·6 mb.
60° S. 80° W.[1]	March 1902	1,000·3 mb.	—
	March 1903	981·0 mb.	—
Laurie Island 61° S. 45° W.	May 1911	1,008·5 mb.	+ 16·8 mb.
	May 1927	983·6 mb.	— 8·1 mb.

The longest cyclonic period of which we have a record in Antarctica was August–October 1940, giving monthly mean pressures of 972 mb. (departure from four-year normal for August — 6 mb.), 973 mb. (anomaly — 6 mb.) and 972 mb. (anomaly — 1 mb.) for the successive months at Bay of Whales. This place had a monthly mean pressure as low as 970·3 mb. in October 1911 and October 1934 (14). In Graham Land the lowest monthly value is 973·9 mb. for March 1935 at Argentine Islands (65° S. 64° W.) (15).

The eight years for which values are available at the southern limit of the Ross Sea in 78° S. show consistently very low pressures in October, pressure being lowest and the variance smallest in that month:

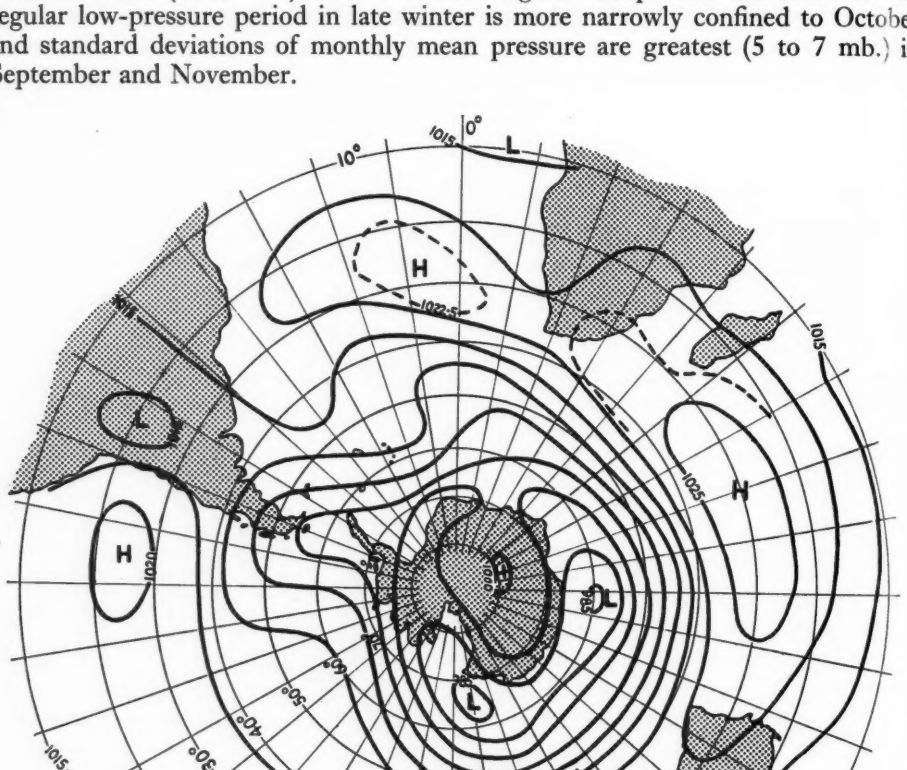
MONTHLY MEAN PRESSURES AT M.S.L. IN 78° S.

Month	Bay of Whales (164°W.)	MacMurdo Sound (166°E.)
October 1902	—	984·7 mb.
,, 1903	—	982·8 mb.
,, 1908	—	984·2 mb.
,, 1911	970·3 mb.	976·0 mb.
,, 1912	—	985·0 mb.
,, 1929	978·0 mb.	—
,, 1934	970·3 mb.	—
,, 1940	972·0 mb.	—

The standard deviation of pressure in that area appears to be of the order of 10 mb. in November—the month between the late-winter low pressure and December–January high pressure—and is almost as great in June when the main

[1] The figures for this point are derived from an early series of daily synoptic charts for the Antarctic Ocean (13).

winter period of low pressure is approaching. February to May and August to October appear as months with much less variance and presumably show greatest regularity of weather type in the Ross Sea sector. The southernmost stations in Graham Land (65–68° S.) show a similar regime except that the more or less regular low-pressure period in late winter is more narrowly confined to October and standard deviations of monthly mean pressure are greatest (5 to 7 mb.) in September and November.



[*By courtesy of NOTOS, Pretoria*]

Fig. 4.—*Average pressure at M.S.L. in mb., September 1953* (after NOTOS, Pretoria, Vol. 3, No. 4, p. 339).

Anomaly patterns of several broad types may be distinguished:

(i) Spells with above normal pressure in most sectors in latitudes south of 60° S.

(ii) Spells with below normal pressure in most sectors.

(iii) Spells with decidedly skew arrangements of high pressure on one side and low on the other side of the pole. An example is given in Fig. 4 (16) which is thought to represent a pattern that is common in late winter.

Case (iii) is often related to a blocking pattern in which an offshoot of the sub-tropical anticyclones develops well southwards in one sector; but sometimes an

anticyclone previously over Antarctica is responsible, by becoming established over the sea ice.

(3) *Cyclonic and anticyclonic weather: evidence of wind directions*

All sectors of the coast of Antarctica show a certain frequency of winds with westerly components, at all heights from the surface up, corresponding to low pressure centred over the continent farther south. Frequencies are commonly between 10 and 20 per cent. At nearly all places in the Antarctic the frequency of westerly winds, low pressure, cyclonic situations and gales is greatest in winter. (This is unlike the central Arctic, but resembles those parts of Greenland nearest to the depressions of the North Atlantic.) Maudheim was an exception: there the percentage of winds with westerly components (mainly S.W.) was 20·4 in the summer half-year (October–March) and 15·9 in winter. This may mean that the sector of Antarctica nearest the Atlantic and Weddell Sea gets a heightened frequency of anticyclones in winter through linkage with blocking highs, which occasionally move into East Antarctica, or at least that something of this nature occurred in the years 1950–2 that Maudheim was occupied.

The variability from year to year is an important point to reckon with as illustrated by the following table:

FREQUENCY OF WESTERLY WINDS

Place	Period	Percentage
Cape Adare (71° S. 170° E.)	Twelve months, 1899–1900	11
	Year, 1911	34
	November, 1899	7
	November, 1911	32
Ships off the coast near 65° S. 165° E.	March, 1947	65
	March, 1948	21

Even Little America on the edge of the Ross Ice Shelf in 78½° S. has had about 30 per cent of its winds from a westerly point over twelve months (1934–5) and Marguerite Bay at 68° S. in Graham Land up to 62 per cent westerly.

Of the first six months for which fully analysed southern hemisphere weather maps were available in 1947 and 1948, January and February 1947 and the period 15 March–14 April 1948 showed anticyclones over Antarctica on 90 to 100 per cent of the days whereas in March and April 1947 this was so on only 40 to 50 per cent of the days.

If we regard this variability as a manifestation of the behaviour of the free atmosphere, topographical and geographical controls are nonetheless very prominent in Antarctica.

Topographical control is most obvious in the funnelling of surface winds by natural drainage channels, which are the natural course for dense cold air running off the ice-cap.

Temperatures on the Ice-Cap

The temperatures on the Antarctic ice cap as in Greenland are certain to be considerably lower than in the free air surrounding at the same height. (— 14·7° C. was the *highest* air temperature measured by Scott or Amundsen on the South Polar Plateau.) Cold, dense air therefore has a constant tendency to run down off the ice cap and be replaced by the surrounding air.

In conditions of light wind under clear skies hollows in the Antarctic inland ice are likely to produce the lowest surface temperatures anywhere in the world. Scott observed — 40° C. in early summer (November 1903) in such a hollow 7,000 ft. up near 77° S. 147° E. Nothing is known of the site at the Soviet advance base Pionerskaya (70° S. 95° E.) at 8,000 ft., but — 62·5° C. was observed in the first month of observations (June 1956) and — 64·0° C. on July 4, 1956—cf. previous lowest in Antarctica — 59·5° C. and the world's lowest accepted reading — 67·7° C. at Verkhoyansk, north-east Siberia.

At the coast of Antarctica tremendous changes of temperature occur within a few hours at points near open water, when a wind from the ice-cap, is exchanged for air from the ocean. Temperatures as high as + 11° C. have been occasionally observed in winter in Graham Land and a rise from —20° C. to a few degrees of thaw has occurred in three hours.

ACKNOWLEDGMENT

I am indebted to the Director of the Meteorological Office for leave to publish this work.

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GEOLOGICAL RESULTS OF BRITISH ANTARCTIC EXPEDITIONS TO SOUTH VICTORIA LAND PRIOR TO 1914

By Dr. W. Campbell Smith, C.B.E., M.C.

Even the briefest review of the geology of South Victoria Land needs to be prefaced by a tribute to the men who did the work in the Antarctic. The story begins with Sir James Clarke Ross who sited the mainland in 1841 and named the two volcanic peaks of Erebus and Terror on Ross Island, and it ends with the work of the men of Scott's Last Expedition and the specimens and notes made by Doctor E. A. Wilson that were found on the sledge at the last camp of Scott's Polar Party. The names of most of these men appear in the account that follows, and others were mentioned in The President's Address (*Advancement of Science*, Sept. 1956). One cannot do

better perhaps than to quote again what Sir Albert Seward wrote of them at the conclusion of his account of the fossil plants collected on Scott's last expedition: 'They have laid a solid foundation; their success raises hope for the future, and will stimulate their successors to provide material for the superstructure.'

Apart from their very considerable contributions to the glaciology and physiography of South Victoria Land and to our knowledge of the recent and local deposits the men of the British expeditions to that sector of the Antarctic continent prior to 1914 had established the existence of the following formations:

1. Volcanic Rocks. Recent to perhaps as old as Tertiary. These occur along the coast from Ross Island by Franklin Island and Coulman Island to Cape Adare with many long gaps; also at two small craters on the mainland.
2. The Beacon Sandstone Formation and associated quartz-dolerite sills.
3. Limestones and limestone-breccias, perhaps of Cambrian age.
4. The Slate-Greywacke Formation of Roberston Bay. Unfossiliferous and possible Palaeozoic or Pre-Cambrian.
5. Several groups of granitic and dioritic plutonic rocks and associated dikes in the Ferrar Glacier, Granite Harbour, and Terra Nova Bay regions, and also in the neighbourhood of Mt. Hope at the entrance of the Beardmore Glacier.
6. Metamorphic rocks forming a Basement Series, bordering the coast in the McMurdo Sound region and abundant in the moraines inland from Terra Nova Bay.

The main lines of the structural geology were drawn by Sir T. W. Edgeworth David and Sir Raymond E. Priestley in their report on the geological results of Sir Ernest Shackleton's British Antarctic Expedition, 1907–9. Their map (Fig. 1) showed that the volcanic centres are related to a main north–south fault running from Ross Island to Cape Adare, and that they probably mark the intersection of this fault by other faults of which the existence is inferred from the topography.

The metamorphic rocks of the Basement Series are exposed *in situ* at many points near the coast and evidence of their further extension inland is afforded by erratics in the moraines of the Terra Nova Bay region. They comprise:

Biotite-gneisses and schists, some of which carry cordierite and/or sillimanite. Many of them are garnetiferous.

Crystalline limestones, seen *in situ* in the Ferrar Glacier region and as great masses in the granites of Granite Harbour.

Para-pyroxene-granulites, found as xenoliths in the granites of Granite Harbour and as erratics in the Terra Nova Bay region and at Cape Royds on Ross Island.

A porphyritic biotite-gneiss, strongly foliated in part, found as erratics but not *in situ*.

Also as erratics in the Terra Nova Bay region are found Graphitic micachists with characters possibly indicating thermal metamorphism of carbonaceous sediments that may well be Palaeozoic.

In the Terra Nova Bay region an attempt was made to ascertain the probable distribution of these several metamorphic groups by analysing the collections of specimens from each moraine separately. This met with some success. The graphitic mica-schists are in the west moraines of the Priestley Glacier and the same moraines contained a carbonaceous facies of the Beacon Sandstone and volcanic rocks that look older than the Tertiary volcanics and may be associated with the quartz-dolerite sills of the Beacon Sandstone.

The Plutonic rocks occur *in situ* in the McMurdo Sound and Terra Nova Bay regions and as erratics also at Cape Adare.

In the region from Ferrar Glacier and the Kukri Hills to Granite Harbour two distinct granites have been recognised. These are an older, grey hornblende-free

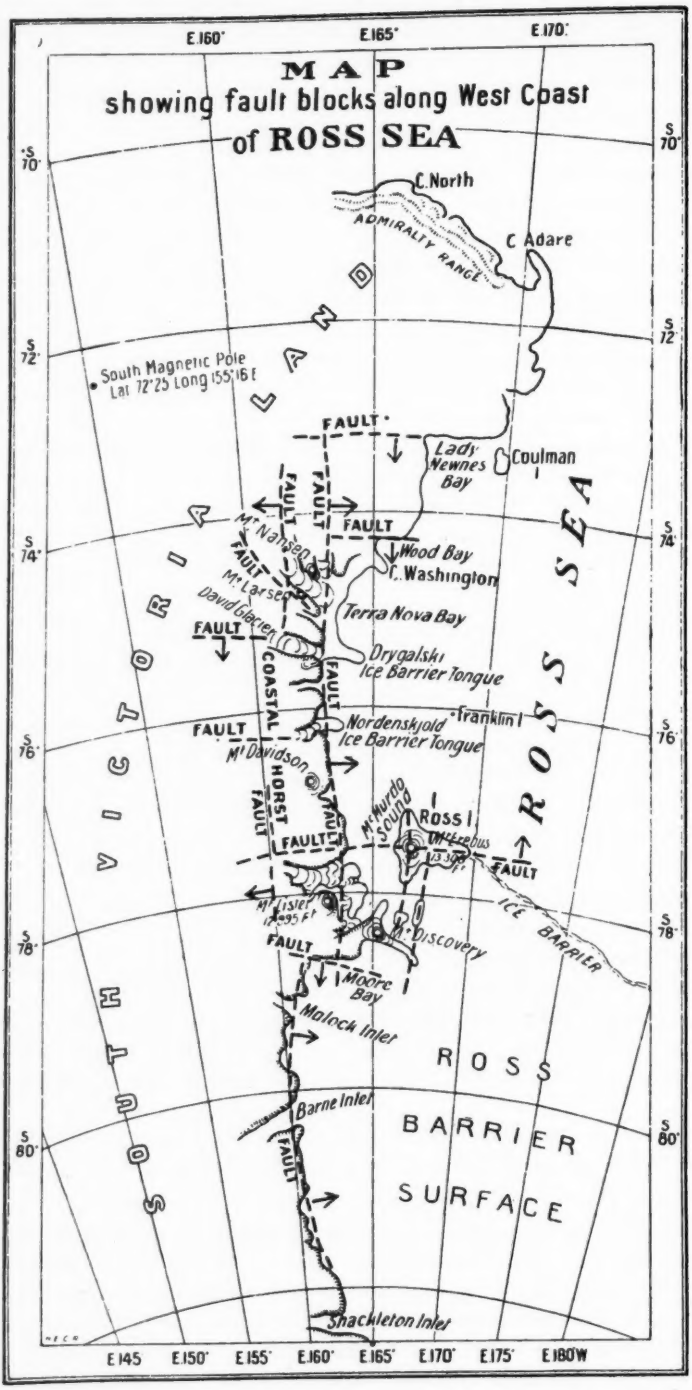

Fig. 1.—Map reproduced from 'Geological notes of the British Antarctic Expedition, 1907–9', by Raymond E. Priestley and T. W. Edgeworth David. Published in the *Report, XI International Geological Congress, 1910.*

granodiorite with associated aplitic dikes; and a younger, pink hornblendic biotite-granite, well developed in Granite Harbour and also described by Sir Douglas Mawson from Cape Irizar. The dikes associated with the younger granite are quartz-orthoclase-porphyries, orthoclase-porphyries, and lamprophyres, some of the dikes being composite. Ferrar believed this granite was younger than the dolerite sills of the Beacon Sandstone, but Debenham interprets the field-evidence otherwise and believes both granites to be older than the dolerites.

The grey granite has been reported from Mount Larsen to the Kukri Hills, a distance of over 100 miles. It may be a great intrusive mass of which the edge is exposed along or near the coast. It is remarkable that observations of the upper limits of its outcrops are rather uniform, and also that it is separated from the overlying Beacon Sandstone by the dolerite sills. In the Granite Harbour region the older grey granite contains abundant xenoliths of metamorphic rocks. A detailed piece of mapping by Frank Debenham at the Flat Iron in Granite Harbour demonstrated a very close relation between distribution of para-pyroxene-granulite xenoliths and development of a hornblendic facies in the granite.

In the Terra Nova Bay region there are biotite-granites and microgranites intricately intermingled with dioritic rocks. Comparisons have been drawn between these and the 'Tingha' granite and others described by E. C. Andrews in northern New South Wales. Priestley's collecting from the moraines of the Terra Nova Bay region also indicates an intrusive complex of dioritic rocks, porphyritic biotite ortho-gneiss, muscovite-bearing granite and tourmaline-bearing pegmatites and aplites in the Northern Foothills and in the block of land between the Campbell and Priestley Glaciers.

The Slate-Greywacke Formation of Robertson Bay behind Cape Adare observed by Borchgrevink and Louis Bernacchi and extensively collected from by Priestley consists of unfossiliferous greenish grey slates and greywackes of unknown, possibly Pre-Cambrian, age. The lithology has been fully described by R. H. Rastall and Priestley.

Cambrian Limestones are believed to be *in situ* below the Beacon Sandstone of Buckley Island on the Beardmore and it was here that Scott called a halt on the polar party's last journey so that a further search could be made for fossils, and notes and sketches made by E. A. Wilson to ensure that the scientific results of that tragic journey should be worth while.

Shackleton's earlier polar party (1908) had found specimens of a limestone breccia in moraines near the Cloudmaker at the entrance of the Beardmore Glacier. The limestone contained fossils identified by Griffith Taylor as *Archaeocyathus* and confidently referred to the Cambrian. This breccia might be a basal bed below the Beacon Sandstone.

The Beacon Sandstone, named by H. T. Ferrar, geologist to Scott's Discovery Expedition, 1901–4, is a vast formation now known to extend from Buckley Island (85° S.), over the Ferrar Glacier region, certainly present in the sources of the Priestley Glacier moraines, probably forming Mount Nansen and perhaps extending to Adelie Land where Mawson found a similar sandstone formation. Inclusions in the lavas on Ross Island show that it extends beneath the volcanic formation east of the coastal fault.

With this sandstone are associated great sills of quartz-dolerite that have been compared with similar sills in Tasmania, where they are of Cretaceous age, and in Natal, British Guiana, and Brazil where they are late Mesozoic.

Fortunately, there is palaeontological evidence of the age of the Beacon Sandstone.

Debenham found fish scales in sandstone at Granite Harbour and Dr. Smith Woodward considered they were Upper Devonian.

Ferrar had found carbonaceous bands in the sandstone but failed to find identifiable plant remains. Shackleton's polar party in 1908 found bands of coal at Buckley Island and some plant remains possibly of a gymnospermous plant, establishing for the first time the existence of an arborescent plant on the Antarctic continent. It was left to Priestley, however, on the northern party in 1911, to find at last fairly well-preserved fossil wood in the Priestley Glacier moraines and Dr. E. A. Wilson succeeded in collecting identifiable fossil plants from Buckley Island. On these specimens Professor Sir A. C. Seward wrote a very important memoir. He identified *Glossopteris indica* Schimp. in Wilson's material and a spore of a new genus *Pityosporites antarcticus*, along with the fossil tree *Antarcticoxylon Priestleyi* in the Priestley Glacier material. *Glossopteris* ranges from the Upper Carboniferous to Rhaetic, but Seward wrote: 'It is . . . not improbable that *Glossopteris* appeared on the Antarctic Continent earlier than in other parts of the southern province. . . .' He concluded that 'the probability is that the beds are Permo-Carboniferous in age. . . .' Thus the fish scales and the plants place the range of the Beacon Sandstone as from Upper Devonian to Permo-Carboniferous.

The Volcanic Rocks, wonderfully developed at Ross Island and at Cape Adare, and represented at two small craters on the mainland in the Ferrar Glacier region, may be of any age from Tertiary to recent.

On Ross Island the lavas represented are basalts, kenytes, and phonolitic trachytes. Many of them show close resemblances with the lavas of Kilimanjaro in Tanganyika Territory.

The lavas of the small craters on the mainland are olivine-basalts.

At Cape Adare, with the exception of one phonolite *in situ* and some phonolitic rocks and alkali-trachytes occurring as 'bombs' in an agglomerate, all the lavas are basalts. Some of them resemble the basalts of Hut Point, Scott's first headquarters. Some of the basalts at Cape Adare contain numerous inclusions of arenaceous rocks which are probably derived in depth from rocks of the Slate-Greywacke Formation below the Volcanics. Somewhat similar inclusions in the phonolitic trachyte of a small cone on Erebus on Ross Island are attributed to the Beacon Sandstone which is thereby shown to underly the Volcanics east of the north–south coastal fault.

Mount Erebus is still active and there the kenytes form the latest flows. Eruptions of Erebus were observed and described by Edgeworth David and Priestley in 1908. The most remarkable product of the recent eruptions is a thin deposit of loose, large crystals of anorthoclase feldspar which strew the crater slopes. Some of them are still embedded in, or partly coated by, vesicular black kenyte-glass. Crystals of similar composition though of slightly different habit are found also in abundance high up on Mount Kenya and in the craters of Kilimanjaro. Their ejection in this remarkable way seems to be a characteristic of kenyte volcanoes of which these three are the only certainly known representatives.

THE GENETICS OF MICRO-ORGANISMS[1]

by

PROF. D. G. CATCHESIDE, F.R.S.

RECENT progress in knowledge of the reproductive processes in micro-organisms has depended solely upon the application of genetic principles, elucidated in higher plants and animals and especially developed for those lower organisms, like some fungi, which are more amenable to ordinary methods of analysis. Understanding of the cytology of the reproductive processes of bacteria, viruses and even yeasts is as yet beyond the powers of the microscopist. True, in bacteria a copulative process has been seen between pairs of *Escherichia coli* and, by electron microscopy, to involve the formation of a short conjugation tube between the pairing cells. But the exact nature of the genetic material remains cytologically unknown. Structures analogous to nuclei and chromosomes have been described in certain bacteria, but, even were the observations certain, and there is much dispute, they fall short of that familiar to cytologists dealing with higher organisms. There is no circumstantial evidence as yet to connect them with the phenomena of inheritance in bacteria. Indeed, our knowledge of the latter ought to warn us that the physical bearers of heredity in bacteria, and viruses, may behave differently from those in higher organisms.

The modern work on bacterial genetics was initiated shortly after it was shown by Beadle and Tatum (1941) that simple nutritional mutants, each determined by single genes, could be obtained very readily in fungi. Briefly, it was known that *Neurospora crassa* would grow and fructify on a medium constituted of inorganic salts, including nitrate, a sugar, water and the vitamin biotin. All the rest of the complicated organic compounds composing its protoplasm could be synthesised from these simple resources by the metabolic activities of the fungus itself. It was further known that other fungi had greater requirements for special growth factors, and it was surmised that it should be possible to obtain from one fungus a whole suite of mutants which had various growth factor requirements additional to those of the common, or wild type, strains. This surmise was brilliantly verified.

One of the first uses to which these mutants were put was in unravelling the pathways of biosynthesis of a variety of essential organic constituents of living matter. The first was a demonstration of a cycle in arginine synthesis, like the Krebs ornithine cycle of mammalian tissues. This hint of the universality of the patterns of biosynthesis in living organisms has very largely been supported by later work, in which *Neurospora* and the bacterium *Escherichia coli* have played a leading part. The present knowledge of arginine synthesis and its relation to proline and glutamic acid, apart from the ornithine cycle, has been inspired by *Neurospora*. So, also, has unravelling of aromatic biosynthesis, especially tryptophane and its relation to nicotinic acid. The mutants have provided tools for taking apart the synthetic mechanisms and, since the genetic mutants may be variously combined and recombined, of putting the synthetic mechanisms together again in new ways so that interactions become accessible to study.

The success in obtaining nutritional mutants in *Neurospora*, inspired Tatum and others to seek for them in bacteria, a group at one time notorious for the paucity of characters which could be used for genetic analysis. Again success followed.

The nutritional mutants (auxotrophs) provide useful tools in the search for

[1] Address delivered to Sections D (Zoology) and K (Botany) on September 3, 1956, at the Sheffield Meeting of the British Association.

sexual reproduction or similar processes in bacteria. It is an attribute of sexual reproduction that different genes brought into the zygote by the fusion of two gametes later recombine in various ways during the process of meiosis which must occur once during each sexual reproductive cycle. Consequently the detection of recombination constitutes evidence for a sexual process; moreover, a statistical study of the classes of combinations will disclose whether there is a chromosome system with linked inheritance, i.e. systems of linkage groups.

Early attempts to apply these principles to bacteria gave unconvincing results, because the characters used had appreciable mutation rates and no significantly greater frequencies of new combinations could be observed from mixed cultures. If, as seemed likely and is known now, the sexual process were generally a rare one, quite special selection methods would be needed. The most effective would be one where only products of recombination are able to grow. A mixture of two auxotrophs plated densely on a medium lacking the growth factors required by each auxotroph may produce prototrophs, with relatively less complex nutritional requirements. The probability that these are produced by recombination of the selective markers, rather than by back mutation, is a function of the relative rarity of the latter event. Such rarity may be achieved by using for the crosses stable mutants or multi-mutant stocks, or stocks which give a specially high frequency of recombination.

Tatum and Lederberg, by repeated treatment and screening of *E. coli*, obtained strains each with two or three nutritional mutations. Each mutation had an independent reversion rate of about 10^{-7}, so that the two or three mutations had a negligible chance of reverting simultaneously in the same cell to produce a prototroph capable of growing without any of the supplementary growth factors. Mixtures of two different multi-mutant strains not only produce prototrophs rather readily but also, by special screening methods, could be shown to produce auxotrophs that combine mutations from the two parental auxotrophs. Moreover, the statistical relationships of the old and new combinations altered in the way expected if the mutant characters were due to genes situated in one linkage group, in one genetical chromosome. The relative frequencies were inverted according to whether the markers were originally together in one parent or apart and one in each parent. In the conduct of such experiments, it is usual to have a system of selective markers to yield a group of relatively prototrophic recombinants, amongst which the statistical relations of recombination between unselected markers may be studied. In properly conducted experiments the unselected (and selected) markers should be introduced into the crosses in all the possible orthogonal combinations, in order to allow for viability differences and for the bias introduced by the enforced recombination between the selective markers.

Certainly, *E. coli* has proved to be more complex and curious than the original, relatively simple, story suggested, but there can be no doubt that sexuality of some kind exists. Further, it is clear that a kind of heterothallism exists, there being two types of strain, F^+ and F^-, the former donating genes and the latter accepting them; respectively they are functionally like male and female. However, F^+ can convert F^- to F^+ without simultaneously causing any other genetic change. The curious feature of sexuality in *E. coli* is that the F^- (the acceptor or female parent) generally appears to contribute the major part of the genes present in a recombinant, the F^+ contributing a minor portion. A further complexity, which gave trouble for some time, was an apparent lack of linearity in arrangement of the loci in one part of the genetic map. Nevertheless, with properly designed experiments, complete consistency with one linear chromosome is attainable (Fig. 1). As Cavalli-Sforza and Jinks (1956) have shown, the unequal contribution of the F^- and F^+ parents to the progeny is explicable by a highly localised elimination of a particular small region (the elimination locus *E*) of the chromosome contributed by the F^+

parent. The disturbing effects on segregation at other loci diminish with remoteness from the elimination locus.

In crosses between F^- and *Hfr* (a special F^+ strain which shows a high frequency of recombination) where one is motile and the other non-motile, it is possible to recognise the paired cells, to isolate them and examine their respective products. Recombinants are frequently found among the progeny of the F^- cell of a pair, but apparently not of the *Hfr* cell. The conjugation appears to permit only a unilateral transfer from *Hfr* (or F^+) to F^-.

Moreover, the conjugation must be maintained for at least a minimum period of time for a given marker derived from the *Hfr* parent to appear in any of the recombinant progeny. This has been established by Wollman and Jacob (1955) by interrupting the conjugation process mechanically. If the coupled pairs of bacteria are forcibly separated in a Waring Blender and plated out on selective media upon which recombinants will grow, the minimum time ranges from about eleven minutes for *Az* to twenty-four minutes for *Gal* (Fig. 1). It is increasingly greater for those loci nearest on the genetic map to the elimination locus. The proportions of *Hfr* markers appearing amongst the treated recombinants increase with time of conjugation, up to maxima which are established by fifty minutes after the commencement of conjugation. The maxima are characteristic for each *Hfr* marker and are greatest for those which appear first in the recombinant progeny. The same

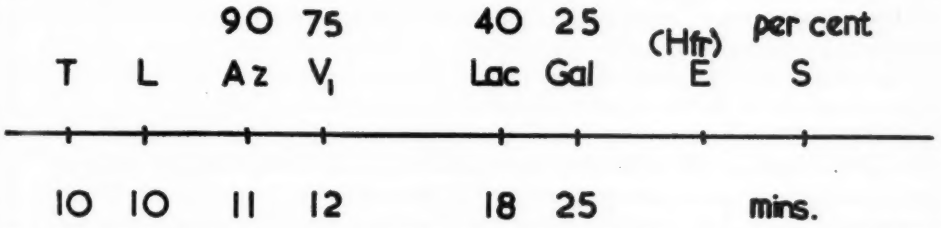

Fig. 1. Map of genetic chromosome of *Escherichia coli* showing, above, the percentage of *Hfr* markers finally appearing in T^+S^+ recombinants and, below, the time in minutes from the mixing of the parents to the first appearance of *Hfr* markers in recombinants. (Data of Wollman and Jacob (1955).)

proportions appear in controls at all times presumably because the pairs remain together when plated. Each maximum represents the recombination occurring between the locus of a marker and the elimination locus. There is in fact an exact parallel between the genetic map and the time of first entry into a zygote.

The mechanism appears to involve the passage of a chromosome from donor (*Hfr* or F^+) parent to acceptor (F^-) parent, starting at a point of origin *O*, and with an increasing amount of the rest of the chromosome passing into the acceptor with the elapse of time. Mechanical treatment breaks off the piece of chromosome which has entered, and in untreated conjugations the donor's chromosome appears to break spontaneously at the elimination locus in most zygotes. In a few, breakage occurs beyond *E*, in the map to the right of it. Genetic recombination appears to occur between the acceptor chromosome and the fragment of the donor chromosome by a mechanism resembling the incorporation which occurs in transduction. It is believed that F^+ activity depends upon a phage-like particle, harboured by F^+ cells and released by them. It is infectious, but not lytic, and it becomes associated with a part (or parts) of the chromosomes of a small proportion of the cells it inhabits. It acts as the vehicle in gene transfer from F^+ to F^- cells.

The situation in *Salmonella* is even more extraordinary. Here a lysogenic strain will release a phage, which can transfer genetic traits from one bacterium to another. Zinder and Lederberg call the process transduction. Each character is transferred independently of all others except for a few cases. Instead, as in *E. coli* conjugation,

of a large chromosome fragment being transferred, in *Salmonella* only a small and apparently randomly chosen fragment is transferred. This fragment commonly embodies only one, rarely a few, known genes. There appear in most cases to be a number of recombinable sites within the locus of each gene that is associated with a distinct physiological or biochemical function. The number of sites in a locus may be quite considerable, as many as a dozen, and usually all have indistinguishable physiological effects. Sometimes two or even more loci are linked, and in some cases there is a remarkable agreement between order in the genetic map and order of action of the genes in a sequence of biochemical steps such as tryptophane or histidine synthesis, as Demerec and others have shown.

These agents of transfer are vastly larger than the nuclei acid molecules, derived from virulent strains, that transform avirulent rough *Pneumococcus* or *Hemophilus* cells to smooth virulent ones of homologous antigenic types. But we cannot exclude the possibility that the underlying mechanism of the action of one cell type on another in these three instances may be similar.

Likewise, we must face the possibility that what in *E. coli* acts as an organ of transfer of genetic material, may by relatively small change become an aggressive devourer of *E. coli* cells like a T group phage. The co-operative dweller and bearer of reproductive messages, the F^+ agent, may indeed not be poles apart from the lytic T phage which eats *E. coli* out of house and home in twenty minutes and multiplies a hundredfold in the process. The relation of provirus and virus seen in higher organisms seems to be repeated in micro-organisms, where they show properties unknown in higher organisms.

Bacteriophage, the virus of bacteria, may infect a bacterium and multiply in it. After a short while the bacterium lyses and releases a large number of phage. In other cases infection occurs, but the phage multiplies only at the same rate as the host. Phage in this condition is termed lysogenic. The balanced relation may be disturbed by radiation or chemical treatments, by mating, and also occurs spontaneously. Then multiplication and lysis occur. The phage produced is detectable by its effects on other sensitive, or indicator, strains. The non-lysing bacteria are said to contain prophage, the latent form of the phage.

One of the best known lysogenic phages produced by *E. coli* is lambda (*Lp* = lambda prophage). In crosses it behaves as though the prophage occupied a locus just to the right of *Gal*, whether tested by ordinary genetic procedure or by mechanically interfering with pairing, as Jacob and Wollman have done. Moreover, in such crosses lambda phage can transduce *Gal* and perhaps other markers. The genetic location of lambda prophage is studied using different plaque mutants of it, e.g. a mutant type giving a large plaque on an indicator strain instead of the normal small one. Each lysogenic phage, indeed, carries with it a number of genetic properties, namely ability to lyse, plaque type and position of prophage on the bacterial chromosome. Each different lysogenic phage has its own specific position on the bacterial chromosome. These prophage loci cluster in two regions, one between *Gal* and the *Hfr* elimination locus and the other around *Xyl* and *Malt*. All those located near to *Gal* are inducible by ultraviolet irradiation, while the others are not.

I shall not discuss reproductive processes further in bacteria nor at all in viruses, except to point out that the essential tools are always those of genetic recombination, using appropriate markers.

Let us turn, instead, to a brief consideration of some other aspects of the architecture of the cells of micro-organisms, especially the participation of the cytoplasm in heredity and development, and the occurrence of heritable variation in the absence of gene differences. The first and most extensive investigations concern the protozoon *Paramecium* (Beale, 1954). Here a relatively large ($0 \cdot 2\mu$) particle, known as kappa, is capable of growth and multiplication in the cytoplasm of

certain genetic types, but not others. Its inheritance is exclusively through the cytoplasm, that is normally it is handed on only by the cytoplasm of the female parent at sexual reproduction.

The cell in which kappa is growing produces an antibiotic, paramecin, which is lethal to any cells in which kappa is not present. Under special conditions a cell lacking kappa may become infected by it and thereafter it produces the antibiotic and is immune to its effects. This is a curious and instructive reversal of the usual effects of parasitism and it is debatable whether kappa should be regarded as a symbiont or parasite. It perhaps indicates one of the less usual, or less known, ways in which a parasitism or symbiosis may evolve. It is analogous to the immunity to homologous phage conferred on *E. coli* by latent (lysogenic) phage.

Other cytoplasmically inherited traits in *Paramecium* are the flagellar antigens. The most significant feature of these is that any given stock is capable of manifesting a range of antigens, though generally only one at a time. Any particular phase is usually persistent and is inherited cytoplasmically in crosses. The antigenic phases can be converted from one to another by manipulation of the environment. The possible range is determined by the genotype, but the actual phase shown is determined immediately by the environment. The different phases appear to be means of adaptation to variations in the environment and provide a model for the comprehension of differentiation and adjustment in multicellular organisms.

In another direction, yeasts and *Neurospora* have given evidence about cytoplasmically inherited particles concerned with respiration. The *petite* mutant of yeasts grows slowly, lacks cytochrome oxidase, succinic dehydrogenase and cytochromes *a* and *b*, but has cytochrome a_1 and an excess of cytochrome *c*. In *Neurospora*, the slow-growing strain, *poky*, shows similar biochemical peculiarities, and evidently has a respiratory system of a special type, so far unknown. Two nuclear gene mutants, with similar effects, are also known. It appears that *poky* is dependent for terminal oxidation upon an enzyme system containing riboflavin; the normal strain contains this system as well as the cytochrome system typical of animal tissues. The cytochrome is destroyed enzymatically in *poky* strains. The significant feature is that a whole group of enzymes, physiologically related in function, are lost or inactivated together. This suggests that they are mutually organised in a special structural relationship, in which a defect in one changes the activity of the whole group.

In conclusion, we may note that the advances secured as a result of the *Neurospora* work are threefold. First, the way is open to a closer knowledge of the physiology of gene action and of the organisation of the cell and its metabolism. Secondly, the whole field of bacterial and viral genetics has become accessible and modes of life and reproduction, unsuspected a few years ago, have already been found. Thirdly, it has become possible to study with greater facility the rare event of mutation, for the large populations possible with bacteria allow experimental procedures that could not be applied to higher organisms.

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ENERGY EXPENDITURE

SECTION I (Physiology) devoted the morning of Wednesday, September 5, to a discussion of 'Energy Expenditure'. Dr. R. Passmore, of the University of Edinburgh, read a paper on 'Methods of determining input and output of energy'. He was followed by Dr. O. G. Edholm, of the National Institute for Medical Research, who described the Calorie expenditure involved in different activities, with examples from sport and athletics. The final contribution, entitled 'Energy expenditure in livestock', came from Dr. J. D. Pullar, of the Rowett Research Institute.

Edited versions of these three papers are printed below in the order in which they were delivered.

METHODS OF DETERMINING INPUT AND OUTPUT OF ENERGY

By Dr. R. Passmore

Energy expenditure is a topic of universal and personal appeal. Everyone, in the evening, after a hard day either at work or on holiday, likes to discuss and expound upon the amount of physical energy which he has expended. Moreover, animal heat is one of the classic problems of physiology. Since the times of Aristotle, physicians and philosophers have speculated on its nature. Scientific studies began when Lavoisier surrounded the cage of a guinea-pig with ice and measured the rate at which the ice melted; since then, physiologists have been attempting to measure animal heat under a great variety of circumstances.

In our time, the subject is of special interest because we have developed machines that are able to carry out much of the hard labour previously undertaken by man and beast. Moreover, our recreations, like our work, seldom involve hard physical effort and so high rates of energy expenditure. The effects of these changes on health are certainly widespread: the high incidence of obesity in all classes of an industrial society is obvious to any observer. These factors are provoking new studies in the problems of energy expenditure.

Energy can, of course, be measured in a number of ways and expressed quantitively in various units. Physiologists find the large Calorie (the amount of heat required to warm one kg. of water at 4°C. one degree) the most convenient unit. All modern work is based on the classical experiments carried out by the American physiologist, Atwater, at the end of the last century. With the aid of the engineer Rosa, he constructed a human calorimeter. This was a small room in which a subject could live for several days. The supply of air could be accurately controlled and measured, and the heat evolved by the subject, both at rest and at work, could be directly measured and calculated from the rise in temperature of the water that circulated in the walls of the chamber. The construction and use of such a chamber was an immense technical achievement that has seldom been repeated. Indeed, as far as I know, there is no human calorimeter in use anywhere in the world today. However, Atwater and his contemporaries successfully established two general principles that enable measurement of energy expenditure to be made much more easily both in man and animals under a variety of field conditions.

First, if the energy content of a diet can be determined and the proportion of that energy which is physiologically available is known, then the rate of energy expenditure of an individual can be deduced from measurements of the energy intake in the food. This, of course, is only true if the subject is in energy balance during the period of observation. The stores of energy in the body as fat, protein

or carbohydrate must remain unchanged. Under most conditions, it can be assumed that a person whose weight is unchanged is in energy balance. A large series of experiments on human subjects in Atwater's calorimeter showed that the physiological value of the energy content of a diet could be obtained by using the Atwater factors:

1 g. of protein is equivalent to 4 Calories
1 g. of fat is equivalent to 9 Calories
1 g. of carbohydrate is equivalent to 4 Calories

These factors are only physiological approximations and vary slightly depending on the nature of the foods consumed and also on individual efficiency of absorption from the digestive tract. They have proved extremely useful in determining the physiological energy value of the diets of man, rats and other animals with approximately similar digestive apparatus. (In ruminants the problem is much more complicated.)

The majority of the studies of human energy expenditure have been based on dietary studies in which food consumption has been accurately measured and the energy value calculated either from tables of the chemical composition of the food or from direct chemical analyses using the Atwater factors 4, 9, 4. Dietary surveys on a household basis are comparatively easy to do. All that is required is to weigh and record all food coming into the household over the period and also to record any changes in the content of the larder and other stores at the beginning and end of the survey. Such surveys are very useful, especially for determining energy requirements of large groups of persons. They do not, however, give any information about individual rates of energy consumption. Further, in any community in which an important proportion of the food is consumed outside the home, large errors may be introduced. Individual diet surveys have been carried out, in which all the food eaten by the subject over a period of days has been taken into account. A high degree of co-operation on the part of the subject is necessary and the technique is difficult. For these reasons individual dietary surveys have never been systematically carried out in any community. A full account of the techniques of dietary surveys has been given in an F.A.O. technical report (3). Within their limitations, studies of dietary intake have proved useful as a measure of daily rates of energy expenditure.

Secondly a measurement of oxygen consumption (a relatively easy procedure) enables energy expenditure to be calculated. This process is known as *indirect calorimetry*. Atwater and his contemporaries proved that one litre of oxygen (the approximate consumption during one minute of walking at a good pace) is equivalent to between 4·7 and 5·0 Calories. The exact value depends on the metabolic mixture (the proportion of fat, carbohydrate and protein being oxidised at the moment). For precise work, this can be determined from the respiratory exchange and the urinary nitrogen. Under normal conditions, the figure of 4·8 Cal./l. of oxygen can be used as a suitable approximation.

Measurements of oxygen consumption have been made under industrial conditions for over fifty years. The subjects breathe in normal air through an inspiratory valve and breathe out through another valve into a large rubber bag (a Douglas bag) in which the expired air is collected. This can then be measured and its oxygen content analysed. The rate of oxygen utilisation can then be calculated. Although a great deal of work has been done with the Douglas bag, its use under industrial conditions is limited by the handicaps imposed by the clumsiness of the bag and its limited capacity (usually 100 l.) which is filled in five minutes of moderate work. Recently German physiologists at the Max Planck Institut für Arbeitsphysiologie (4) have successfully overcome these difficulties by developing an apparatus originally tried out by the Swiss physiologist, Zuntz, over fifty years

ago. They have designed a light portable respirometer, which measures directly the volume of the expired air, and simultaneously diverts a small fraction (0·3 to 0·6 per cent) into a rubber bladder for subsequent analysis. This instrument, which weighs less than 4 kg., can be worn on the back like a small haversack and greatly facilitates investigations under a variety of conditions. With the use of the Max Planck respirometer, measurements of rates of energy expenditure at work, in industry and the home, and at leisure and during active recreations, have been made by many workers. Passmore and Durnin (6) have collected together a large number of records of rates of energy expenditure obtained by workers using either the Douglas bag technique or the new respirometer. The Max Planck respirometer was designed originally for use in industry and long experience with it has convinced me that it is thoroughly reliable under all normal industrial conditions. At very high rates of work (involving ventilation rates over 50 l./min.), the meter tends to under-record, and it also provides a definite resistance to respiration. It is, therefore, not suitable for studies of very hard work or strenuous athletics. Fletcher and Wolff (2) have devised an ingenious new respirometer which should overcome these limitations.

In practice, however good the apparatus, it is only possible to measure oxygen consumption rates over periods up to one hour and usually over much shorter times. To determine daily rates of energy expenditure it is necessary to find out how a person spends his time throughout the whole twenty-four hours. By a combination of indirect calorimetry and a complete recording of the pattern of daily activities, it is possible to arrive at a measure of daily energy expenditure rates. Four such studies in Britain have been already published. The first was a pilot study of a group of Edinburgh University students who had a life arbitrarily restricted in the laboratory. The second (5) was a study of coal-face miners and clerks at a colliery in Scotland. In the third (1), officer cadets at Sandhurst were the subjects. In each of these surveys simultaneous dietary studies were made and, in general, a very good agreement was found between the estimates of daily energy expenditure rates by measurements of food intake and by the combination of indirect calorimetry and the recording of activities.

We would not pretend that it was easy to get an accurate record of the daily activities of such energetic young men as the coalminers and cadets whom we studied. The records at work were made by independent observers, and during off-work and recreations we relied in large part on the recording by the subjects themselves. The subjects whom we chose for study probably presented the maximum of difficulties because of the fact that they were very active men. The recording of the activities of sedentary men and women would be a much easier task.

The technique which we have developed has been shown to be an alternative method to the now standard dietary survey technique for assessing daily energy expenditure rates. I cannot claim that it is not time-consuming and expensive in the number of assistants required, but in certain circumstances it might be easier than individual dietary studies. The order of accuracy is probably similar. Estimations of the calorie intakes in recorded diet surveys usually have a coefficient of variation of just over 15 per cent. A coefficient of variation of the same order has been found in the individual rates of energy expenditure. Measurements of daily energy expenditure rates are being made in the Armed Forces both in Britain and the U.S.A. A study of troops in the Chinese National Army stationed in Taiwan has already been published. Similar studies will probably be made in various other occupations. In particular, we know little of the effects of modern life and the introduction of domestic appliances on the energy expenditure of housewives.

In investigating energy imbalances, particularly obesity, measurements of rates of energy expenditure are an essential addition to measurements of dietary energy

intake, if the nature of the weight loss is to be determined. In practice, we have found it easy to determine daily energy expenditure of patients, put on a fixed daily exercise schedule.

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ENERGY EXPENDITURE

By Dr. O. G. Edholm

Dr. Passmore has described the techniques by which the input and output of energy can be determined, and the principles on which these techniques are based.

The results of investigations in which such measurements were made should be of interest to others than physiologists. It is, for example, necessary to know the range of energy expenditures in a population if an equitable ration scale is to be drawn up. In the last war, allowance had to be made for the different categories of the population, children, expectant and nursing mothers, sedentary, moderate and heavy workers. These categories, as regards various grades of work, were necessarily broad and in some respects arbitrary. The extension of accurate calorie expenditure studies can provide the basis for a more satisfactory assessment. There are many problems which have not yet been adequately studied, and in particular the effects of climate and of race need further investigation. War-time studies on troops showed that there was a relationship between temperature and food intake, the lowest being in hot climates and the highest in the cold. There is some doubt concerning the application of these findings to populations normally living in any particular climatic zone, especially in cold regions. Canada, parts of the United States, and Scandinavia, all have low environmental temperatures for a substantial part of the year. However, there is a high standard of living in these countries, and a great number of houses have central heating. So the population is only exposed to cold for short periods and lives warmly indoors for the greater part of the day.

Although there have been valuable studies of nutrition in the tropics, more work is needed to measure the level of energy expenditure of people living in the heat. There are indications of considerable racial differences, and it is unlikely that the food standards of temperate zones are applicable to all regions.

Parallel measurements of food intake and energy expenditure are considerably more useful than either one or the other by themselves. However, such measurements need to be made over a period of several days, and on several individuals, as the day-to-day variation in expenditure and intake can be very considerable, even when activities appear to be uniform. The variations between individuals doing the same kind of work can also be surprisingly large. In recent studies on Army recruits, during training, we have found differences to be much higher than predicted. The young men who were studied were of similar age, and were receiving the same food and carrying out the same military training. But food intake showed a range from 1,500 to 7,000 Calories. Energy expenditure does not vary quite as much, but the range is from 2,000 to 5,000 Calories. It is for this reason

that surveys based on studies lasting for two to three days may give misleading results. On the other hand, it is difficult to continue such studies for very long without undue interference with the subjects and, equally important, fatigue on the part of the investigators. A week is probably the shortest useful period, and six subjects at least should be included.

One of the interesting findings has been that the correlation between food intake and energy expenditure is very low when calculated on a day-to-day basis. Agreement becomes much closer when the average intake and output for a week are compared. As mentioned above, the levels of energy expenditure vary very greatly from day to day and also between subjects, even in those leading relatively uniform lives. There have been many measurements of the Calorie cost of particular activities, but not many in which the total energy expenditure for the whole day has been measured. Nevertheless, the levels of energy expenditure in different occupations can be computed from a knowledge of the Calorie cost of each particular activity, together with a more or less detailed time and motion study covering the whole day. In this way the energy expenditure of different occupations in this country has been calculated and it probably ranges between 2,000 to 5,000 Calories per day, with an average for the country as a whole of about 2,600 Calories. It would appear that there are very few occupations in which the daily expenditure exceeds 4,000 Calories, and it is likely that it is only exceptional people who can maintain higher levels indefinitely. On the other hand, the potential maximum energy expenditure is much higher; in any survey one may find an average figure of 3,000 Calories for a particular individual, but he may well reach 6,000 Calories on some special day. When the activities of the day are studied in detail and measurement is made of the energy expenditure involved in any special occupation, the range is found to be very great. If this is expressed in terms of Calorie/minute it will range from approximately 1 Calorie for sleeping to 15 Calories/minute for running upstairs. In terms of oxygen consumption, 1 litre of oxygen burnt yields 5 Calories, so running upstairs costs 3 litres of oxygen/minute. Most healthy young men and women can attain such a level of oxygen consumption, but very few can maintain it. If energy expenditure stayed so high for the whole twenty-four hours, it would be equivalent to 21,600 Calories.

Great athletes can achieve even higher levels of oxygen consumption, and Åstrand has recently published an article showing that champion ski-runners can have an oxygen intake of over 5 litres/minute, which is equivalent to 25 Calories/minute. Such tremendous levels of energy expenditure can only be kept up for quite short periods, to be measured in minutes not hours. It would be of great interest to determine the time for which particular levels of physical activity can be sustained, including those which can be continued indefinitely.

It is clear that the greater the energy expenditure the shorter the time it can be maintained. Studies of athletics and sports provide a useful source of information. The normal duration of a game provides a reasonable measure of the level of energy expenditure; it will be found that a game of squash may last twenty minutes, as compared with football, which usually is one and a half hours, or cricket, which may continue for five days. The energy expenditure in squash has been estimated to be about 10 Calories/minute, and in football about 8 Calories/minute. Fletcher has computed from a time and motion study that the energy expenditure of a Test cricketer is of the order of 2·5 Calories/minute.

Channel swimming involves a great feat of endurance, as a relatively high level of energy expenditure has to be maintained for many hours, and it has been calculated that crossing the Channel may cost from 9,000 to 12,000 Calories. So far, this is the largest expenditure which has been recorded in the field of athletics, and it seems unlikely that it will be surpassed. What is of great practical importance is to determine the level of work which can be maintained daily for indefinite

periods, without causing harm. Lehmann and Müller suggest an average level of 5 Calories/minute for an eight-hour working day represents probably the upper limit. With allowances for the heat production during the rest of the day, this amounts to 4,300 Calories/day.

Measurements made on coalminers show that the average expenditure was of the order of 3,600 to 3,800 Calories/day. The table given below represents very approximately the levels of energy expenditure which can be sustained for particular periods of time. The essential point is to emphasise that everyone has a considerable potential range of activity; it is not apparently possible to keep going at more than a fraction of the maximum, indefinitely.

The analysis of the activities of the day, even of very active young men, shows that the greater part of the twenty-four hours of the day is spent not in the upright position. Including the time spent in bed, at least fourteen hours are passed lying or sitting. As Dr. Passmore has pointed out, with the increase in mechanisation, more and more people spend the greater part of the day sitting. We may conclude that man is no longer an upright animal.

Levels of energy expenditure which can be sustained for particular periods of time

25	Cal./min.	5 minutes
15	,,	1 hour
10	,,	10 hours
5	,,	2–3 days
4	,,	10 days
3	,,	Months
2·5	,,	Indefinite for healthy men
2·0	,,	Average for population

ENERGY EXPENDITURE IN LIVESTOCK

By Dr. J. D. Pullar

During recent years measurements of total energy expenditure in livestock have been based almost entirely on indirect methods, in which the energy output is calculated from oxygen consumption and carbon dioxide production, on certain reasonable assumptions about the metabolic processes occurring within the subject. Energy expenditure can also be measured directly by purely physical methods, as was the case in the first experiments of this nature, those of Lavoisier (1777).

Indirect methods involving gas analysis were introduced by Regnault and Reiset (1849), and they attained much wider use than direct experiment, partly because the initial cost of the apparatus required is much lower, but mainly because the application in practice is generally more convenient. Direct calorimetry would be the procedure of choice in many experiments, particularly with livestock, since, for example, it is difficult to train animals to wear breathing masks, but it was not until very recently that a direct calorimeter was devised which could compete at all successfully with indirect methods.

The classical types of direct calorimeter had many disadvantages in addition to that of high initial cost. They were difficult to operate; some required several experienced operators continuously in attendance, and, at the end of an experiment, many calculations and corrections had to be applied to the data. Also, perhaps most

important, the speed of response was generally very slow, the instrument taking up to an hour or so to respond fully to a change in the thermal output of the subject, which also means that a long preparatory period was required at the beginning of an experiment before reliable measurements could be obtained.

There is no doubt, however, about the accuracy of the older calorimeters, and the validity of the use of indirect methods depends primarily on the close agreement in results obtained by two approaches. Direct and indirect measurements were sometimes combined in a single instrument such as the famous Atwater–Rosa–Benedict respiration calorimeter, this facility giving a valuable cross-check when the results obtained by either method singly were in doubt.

Since the days of this instrument, the trend of calorimeter design has been in the direction of automatic operation and increased speed of response. Many different types were devised between 1900 and 1940 for humans and for different animals, but few of them survive in regular use today.

Interest in direct calorimetry was greatly stimulated soon after the last war, when Benzinger and Kitzinger (1949) published the first reports of their gradient-layer type direct calorimeter. This instrument has a greatly increased speed of response compared with older types, and for the first time it became possible to attain completely automatic operation, an advantage most fully realised when working with livestock as opposed to human subjects, since with animals the calorimeter can be left completely unattended.

The theory of the gradient layer calorimeter and its application to human subjects has been fully discussed by Benzinger and Kitzinger (1949). A similar type of instrument has been in use for some time now at the Rowett Research Institute for work on animals of live weight up to 50 lb. A larger calorimeter for animals up to 250 lb. is under construction. With livestock it is not practicable to attempt detailed partitioning of heat losses into the various channels, and therefore our animal calorimeter is a simplified version of the Benzinger model for humans.

The calorimeter can be divided into two functional parts, the animal chamber which measures the sum of the radiative, convective and conductive components of the heat output of the animal, and the ventilatory circuit in which the airstream is routed through various units which measure the evaporative losses.

The animal chamber in our instrument is built with a thick copper outer wall and a thin copper inner wall. Between these walls lie the gradient layers, which contain over 6,000 pairs of series-connected thermojunctions. These thermocouples measure all heat flowing out of the chamber into water-jackets which determine and maintain constant the reference temperature of the gradient layer. The operating temperature can be held at any desired value above about + 10°C. The layer material is only 0·03 inch thick, so there is little resistance to heat flow, and the inner wall temperature is only fractionally higher than that of the water-jackets. The number of thermocouples is, however, sufficiently large to produce an e.m.f. adequate for direct application to commercially available recorders.

The animal is housed in a cage-like container which slides into the animal chamber, and has provision for disposal of urine and faeces. Liquid and solid foods can be introduced without interrupting operation, light is provided from outside through perspex light guides, and ventilation at a fixed humidity value can be adjusted in relation to the requirements of the animal.

The ventilatory arrangements are of the open circuit type. Air is first humidified if necessary, then cooled to a low temperature at which point saturation must occur. The airstream is next heated to calorimeter operating temperature before passing through the animal chamber, and it is then cooled down again to the low temperature at which it was previously saturated. During the second cooling, condensation occurs of all the water vapour picked up in passage through the animal chamber. The condensing vapour gives up its latent heat to further sets

of gradient layers, and we get a second e.m.f. which is proportional to the evaporative heat losses of the animal.

The final output of the calorimeter thus consists of two e.m.fs., the sum of which is proportional to the metabolic rate of the subject. Instrumental errors, about $\pm$ 15 Cal./day, are mainly independent of the animal's weight, and therefore the accuracy increases with increasing size of animal. At 35 lb. live weight and above, a precision of $\pm$ 1 per cent can be achieved. This figure applies to the measurement of total heat losses only. Chiefly because of the evaporation from urine and wet faeces inside the animal chamber, the partitioning of heat loss into evaporative and non-evaporative channels is less precise.

A multi-pen potentiometer gives a permanent record of the data, and it is normal practice to leave the calorimeter running unattended, particularly during the overnight period. Integration of the results, and conversion from electrical to thermal units, can be accomplished at leisure with a special form of planimeter.

Apart from feeding and necessary initial adjustments, the calorimeter is completely automatic. In work on the physiology and nutrition of livestock this is a very great advantage, and long-term experiments can be tackled which would require a prohibitive amount of labour with a classical type of calorimeter. Owing to the greatly increased speed of response, minute-to-minute fluctuations in heat output can be followed, which makes the gradient layer calorimeter equally suitable for short-term experiments in which relatively rapid effects have to be studied.

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INDUSTRY AND TECHNICAL PROGRESS

by

C. F. CARTER and B. R. WILLIAMS

(*A review of the Report of the Science and Industry Committee*)

At their meeting in Belfast in 1952 the Council of the British Association decided to appoint a Committee 'to study the problems of speeding up in industry the application of the results of scientific research'. Subsequently the Royal Society of Arts and the Nuffield Foundation lent their powerful support to the proposed investigation, and finance was supplied by the Conditional Aid Scheme through the Board of Trade and the Department of Scientific and Industrial Research. A skilled staff were appointed, and the direction of their work was entrusted to Professor C. F. Carter of the Queen's University, Belfast, and to Professor B. R. Williams of the University College of North Staffordshire, Keele. This report represents the first-fruits of the investigations, which have covered a wide field. The Committee have wisely left the responsibility for it to the authors, contenting themselves with general guidance and help.

The book will be of great value to those members of the British Association and others who have given no deep thought to the complex problems of the application of science to industry. For instance, it may dispose, once and for all, of the saying, still common among many scientists, that the genius of British fundamental research is frustrated by the narrow conservatism of business men. As Sir Lawrence Bragg has remarked, 'We all make these general statements lightly and are then hard put to it to justify them'. Whatever truth there may have been in the statement formerly—and there was never quite so much truth in it as scientists have alleged—it is not true today. Times have changed. British industry, speaking generally, is becoming highly scientific and progressive, and would become more so if the supply of scientists and technologists were adequate to meet the still rapidly growing demand.

The authors devote a chapter to the popular topic of the shortage of 'trained men and women' and conclude (in their preface) that they 'are satisfied neither with the effectiveness of the British educational system in its work of selection and training, nor with the distribution of trained talent between industries and firms'. They are sure that 'a national effort is needed, to which scientists, business men and educationalists must all contribute'. There will be general agreement with this remark, but it is not very original. A national effort is being made, and business men are contributing to it to a great extent. The number of university students of science and technology has doubled since the war. Great attention is now being paid to the well-being of technical colleges, and the number of boys and girls seeking a scientific education at school is rapidly increasing. The shortage of school teachers of science is certainly alarming; but some may say that the real handicap to progress lies in the 'narrow conservatism' of the leaders of education. However, the climate of opinion is changing, and the change will be hastened by more public discussion of the points at issue.

In other respects the authors are not so definite in their views. For instance, to examine dispassionately the statement that 'the scientist is needed in the Board Room as well as in the laboratories of modern industry' they have studied the profit records of 500 public companies quoted on the London Stock Exchange. They find that the fifty firms with the worst profit record had directors whose known technical qualifications were below the average of the whole 500, and the fifty with the best profit record had directors with qualifications above the average. A close study of

110 firms leads them to the conclusion that the majority of progressive firms have scientists on their Boards, and that the positive correlation between progressiveness and the presence of scientists on the Board is most marked in the modern scientific industries. Yet they hesitate to conclude that there is a causal relation between progressiveness and scientific direction because they can point to unprogressive companies which do have a scientist on the Board and progressive companies which do not.

They are still less definite on the influence of high taxation on technical advance. It is true that high taxation does not impede industrial research; on the contrary it may encourage it, for all money spent on research inside or outside a business is now regarded as a normal business expense and is untaxed. But when it comes to a decision to put the results of research into practice the picture is very different. Most new processes are financed out of taxed profits. If the process is a failure the whole loss is borne by the company; if it is a success half the profits are taxed away. This makes many firms hesitate to take the kind of risk which ought to be taken in a time of rapid technical advance. The authors believe that notwithstanding this most firms have been able to provide for their development plans. Of 138 firms for which information was available only nineteen reported that lack of finance was a definite hindrance; but they were on the whole the most progressive firms. The authors rightly remark that 'there are few subjects more difficult to analyse with clarity and accuracy' than the effect of high taxation and consequent shortage of money for development; but it is also true that there are few subjects on which it is more important to reach the right conclusion in the present state of the country's economy.

These are only a few comments on a book which should be widely read and discussed. Some of the most interesting information which the authors have collected has been obtained from the case studies of particular firms and industries. It is pleasant to record that the large majority of the 269 firms visited by the research staff were most co-operative and helpful. The authors have certainly invited criticism in many respects. So much the better, for it is only through the clash of opinions that truth will be approached. Sir Winston Churchill once remarked that 'when one leaves the fields of platitudes one enters the fields of controversy'. Let there be as much controversy as possible.

It is to be hoped that the authors will be encouraged to continue their valuable and important work.

H. T. TIZARD

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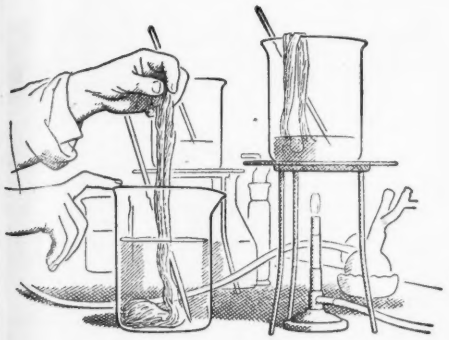

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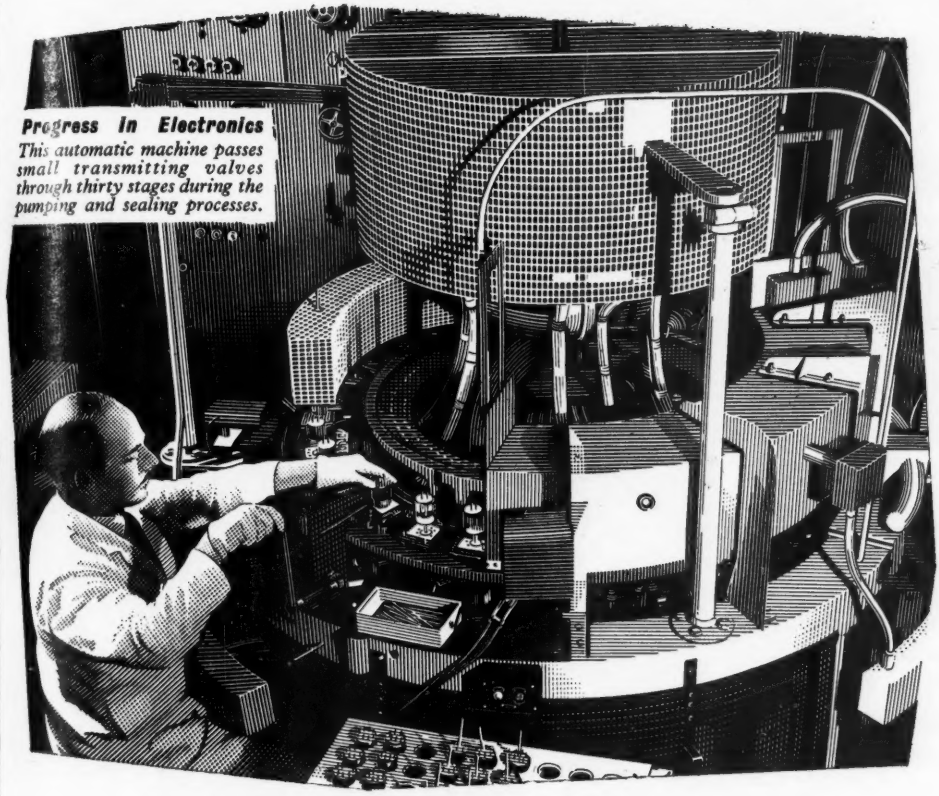

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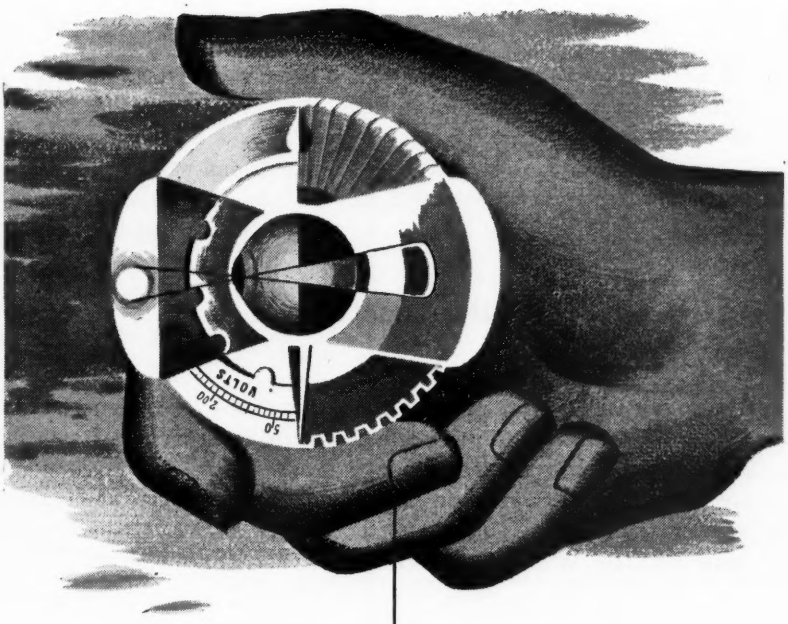

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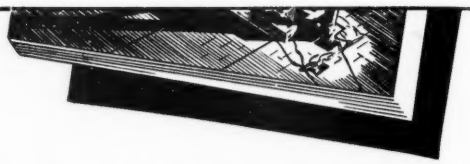

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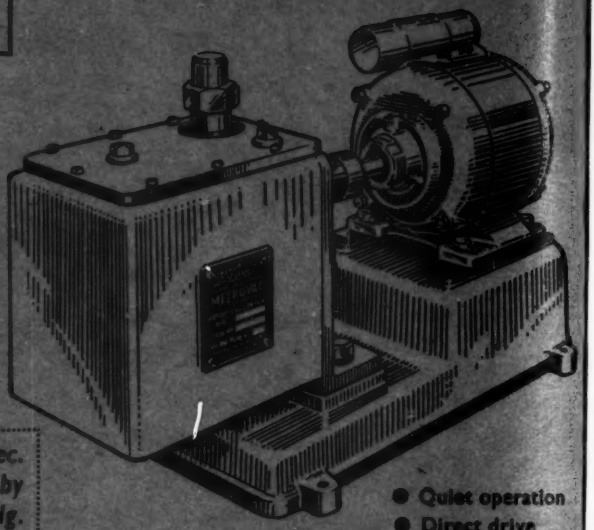

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